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SOCIOECONOMIC STATUS AND INTELLIGENCE: A CRITICAL SURVEY

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The question of the precise character of the relationship between certain socioeconomic variables and intelligence test score comprises one of the most persistent and perplexing problems in the entire field of mental testing. Shortly after Binet began his labors on the construction of an instrument for measuring mental ability, he became aware of a positive relation between social level and intelligence.[1] This result has been confirmed many times since Binet's early work, has been rediscovered with every revision of the Binet up to and including Terman's latest (**54**), and may stand as a generalization which no one has managed successfully to refute.

We are confronted, therefore, with a fact. Wherever large groups of children have been measured by use of the standard tests of intelligence, it can be shown that socioeconomic status is one of the variables that enters into the determination of intelligence test score. On this point there is general agreement. Beyond this fact, however, we are faced with major questions which are still subjects of contro-

[1] Decroly and Degand (**18**), in one of the first practical uses of the Binet-Simon scale, found that children in a private school in Brussels were all either average or advanced for their age, with no case of retardation whatsoever. Binet (**6**) very correctly attributed the differences between the results of Decroly and Degand and his own data to the fact that the cases of the former were all children of the well to do, whereas the Paris group of his own studies were of the lower middle class. From this fact he concluded that social status must be closely correlated with mentality, and made some attempts in his later revisions to allow for this apparent difficulty.

versy, with reference to which the adherents of one systematic position or another have tended to take vociferous sides. How much of a rôle does this variable play in determining IQ? Which of the great variety of social data which accrue to a given individual in our present complex society correlate positively with intelligence? What bearing has this result upon the ubiquitous nature-nurture controversy? What implications has this generalization for education, for government, for the very mechanism of democracy itself? These basic questions have brought with them a host of subsidiary problems which have provided stimulation for much research. Attempts have been made to discover in what manner and to what degree the various social levels grade off in intelligence. There have been efforts to predict the IQ of the child if we know the social level of the parents. Researches have been conducted to discover whether IQ changes with translation from a poor to a good social environment, and to study the effect of such variables as age, length of residence, initial IQ, height of level of new environment, and so on. Questions have been raised as to the precision of our measures of social environment, bringing with them efforts to construct objective scales. And finally, the clinician has debated the question of the emphasis he is entitled to place upon this factor in interpreting IQ differences, where considerations of guidance, diagnosis, and prognosis are of paramount importance.

The challenge of these still incompletely settled questions, combined with the existence of a very large body of research literature bearing upon these problems—literature which is sometimes contradictory and rarely decisive—makes it imperative that some order be brought into the relative chaos of this field. The writer feels, therefore, that the time is ripe for an extended critical discussion of this family of problems, a discussion which should prepare the way for really crucial research. The purposes of this paper, then, are (1) to examine the precise character of the relationship between socio-economic status and intelligence as it is treated in the research literature, (2) to evaluate the techniques which have been used in study of the problem, and (3) to interpret the relationship on the basis of all of the available evidence bearing upon it.

What Are the Facts?

A large number of investigators of the distribution of intelligence in the general population have found occasion to analyze their data

in terms of socioeconomic status.[2] The precise numerical size of the relationship differs somewhat from investigator to investigator, varying with the sample tested, the geographical region visited, the test used, and the type of social classification employed, but the same general finding is confirmed in all these thirty-odd studies. Apparently, whether the research is carried on in this country or abroad, it appears to be a fact that intellectual proficiency, as determined by score on standard intelligence tests, grades off with social level. Granting this general finding, with which we shall attempt to deal critically later, it is important to establish the size of the relationship. Terman and Merrill (54) present a typical spread, which they obtain from the standardization group they utilized in their latest revision of the Binet. Since this group is one of the largest and most carefully selected of those found in the studies under consideration, and is also quite representative of these studies, it will be convenient to present their data in full (Table I).

TABLE I

MEAN IQ'S OF CHILDREN ACCORDING TO FATHER'S OCCUPATION

(Adapted from Terman, L. M., and Merrill, M. A., *Measuring Intelligence.* Houghton Mifflin Co., 1937.)

Occupational Group	% of Total Cases	Mean IQ's
I. Professional	4.5	116.2
II. Semiprofessional and Managerial	7.8	111.9
III. Clerical, Skilled Trades, and Retail Business	25.5	107.5
IV. Rural Owners	14.9	95.1
V. Semiskilled, Minor Clerical, and Business	31.4	105.0
VI. Slightly Skilled	9.4	98.0
VII. Day Labor, Urban, and Rural	6.6	96.1

Total N=2757

[2] If we limit ourselves in this section of our paper merely to those who have attempted only to discover the extent of variation in score with variation in social grouping—leaving for later consideration those who have tried to tease out the reasons for the relationship—we find that a list of the more important studies includes Decroly and Degand (18), Binet and Simon (6), Morlé (44), Hoffman (32), Weintrob and Weintrob (56), Yerkes and Anderson (63), Terman *et al.* (53), Terman and Merrill (54), Bridges and Coler (8), Yerkes (Ed.) (62), Pressey and Ralston (45), Book (7), Dexter (19), Haggerty and Nash (30), Chapman and Wiggins (15), Sandiford (47), Bear (5), Collins (17), Goodenough (28), Duff and Thomson (20), Fukuda (25), Arlitt (1), Jensen (33), Jordan (34), Russell (46), Chauncey (16), Stroud (52), Sirkin (48), Kornhauser (38), Macdonald (43), and Cattell (13).

A compilation of all of the studies which deal with similar groups yields a comparable result.[3] In general, children of the lowest urban social group (unskilled common laborers) *average about twenty points lower* than do children of professionals (the social group at the top of the IQ scale). Terman and Merrill hasten to assure us, as do most of the workers in this field, that these values indicate differences between *means,* and that considerable overlap exists throughout all the distributions. It is this twenty-point difference between means, then, with which this study is concerned, and which has provided the basis for endless theorizing about socioeconomic differences in intellectual ability.

TABLE II

CORRELATIONS BETWEEN INTELLIGENCE TEST SCORE OF CHILDREN AND SOCIOECONOMIC STATUS OF PARENTS

	r	N	Criterion of Social Status
Chapman and Wiggins (15)	+.32	632	Chapman-Sims Scale
Stroud (52)	+.25	1057	Tax Assessment
Burks (12)	+.42	104	Whittier Scale
Chauncey (16)	+.21	113	Sims Scale
Sirkin (48)	+.39	682	?
Fukuda (25)	+.53	257	Whittier Scale
Leahy (41)	+.45	194	Leahy Scale
Terman *et al.* (53)	+.40	492	Teachers' Estimate

A small number of studies have used correlational methods in attacking the problem of socioeconomic status, although undiscriminating use of these techniques is dangerous in this field.[4] The magnitude of the r's found varies considerably from study to study, although all are positive. Below are shown examples taken from the more important studies, listed by investigators (Table II). In this table it is apparent that the lowest r is +.21, and the highest, +.53.

[3] A notable exception to this generalization is afforded by Cattell (13). Using a test which he constructed himself, and which he claims is more "saturated with g" and less affected by environmental variables than is the Stanford-Binet, Cattell reports a spread much greater than Terman and Merrill's data indicate. He finds a mean IQ of 78 for "factory workers," and, at the other extreme, a mean of 151 for secondary school teachers. That these figures do not merely represent a wider range of social differentiation in England is attested to by Duff and Thomson (20), who report a spread almost identical with Terman and Merrill's, using a test comparable to the Binet. Reasons for Cattell's finding will be indicated in a later section of this paper.

[4] The difficulties standing in the way of a straightforward use of the product-moment correlation coefficient are various and will be discussed below.

Whereas these coefficients are not high, they appear to indicate some degree of relationship between the factors measured. Assuming a linear relationship, and using an r of +.45 as a safe estimate, it should be possible to predict the IQ of a child, knowing the socioeconomic status of the parents, with an accuracy some 10% better than if we simply guessed. It must be conceded that this degree of predictive efficiency is certainly not high enough to justify the use of socioeconomic status as a practical indicator of level of intelligence.

The data presented in Tables I and II raise the problem before us very clearly. To repeat, what are the factors which make for this mean difference of 20 points between laborers' children and the children of professionals? If we consider the correlations, what factors are common to both social level and intelligence test score which make for some degree of covariation? In finding our answers we shall have to consider researches relatively far-removed from the immediate field of social level and intelligence. Problems important for our consideration are (1) the question of the constancy of the IQ, (2) the effect of environment on the IQ, and (3) the question of the meaning of the standardization procedures commonly utilized in constructing tests of intelligence. In essence we shall set ourselves the task of trying to demonstrate that *all* of this troublesome twenty-point difference in mean IQ is attributable to factors extrinsic to innate endowment.

Techniques of Study

Before we attempt to deal with the problems set at the end of the preceding section of this paper, it will be necessary to raise certain methodological considerations. It is clear that we are concerned with examining a relationship between 2 variables: (1) 'intelligence' and (2) 'socioeconomic status.' Although much remains to be discovered about the first of these variables, it must be admitted that we possess a good deal of information concerning it. Instruments purporting to measure it—the standard tests of intelligence—are in very wide use, the details of their construction are accessible, and there is considerable literature on the assumptions underlying their application and interpretation. We have a fairly clear notion of the meaning of a ten- or twenty-point difference in IQ, with reference, of course, to the particular part of the range under consideration. There is good general agreement on the IQ ranges that are described by such terms and phrases as 'very superior,' 'inferior,' 'normal,' 'borderline,' and 'dull.' In short, although the theory in

this field is still unsettled, there are at least common instruments and a common language.

Very little of the above can be said for the second of our 2 variables, 'socioeconomic status.' We possess no 'standard' and carefully worked up measures of this variable. The studies under analysis in this paper all agree in using the same well-known and widely discussed tests of intelligence, but they vary widely in the procedures they employ in measuring social level. It is quite clear that we cannot sensibly talk of measuring social level unless we are familiar with the degree of precision of the tools used in the measuring process. We shall find our understanding of the general relation enriched, then, if we pause briefly for a consideration of the methods used in measuring socioeconomic status.

Most of the early investigators [5] tell us little more than that they compared the scores of the children of the same age found in "poor neighborhoods and in rich neighborhoods." Nowhere is there indication in these early studies of any serious precautions against errors of sampling, or any ambitious attempt to construct a scale which would give some continuous measure of social level. The procedure was quite rough, admittedly exploratory, and served merely to indicate the existence of the problem. With increasing awareness of the gravity of the problem came attempts to render description of social level more precise. So we find Terman (**53**), in his first revision of the Binet, asking the school teachers of the children making up his standardization group to classify these children into 5 social groups—very superior, superior, average, inferior, and very inferior—on the basis of what information they possessed of the parents. Arlitt (1) used the same technique and the same classification.

Very soon came attempts to construct scales giving a measure of variation from low to high status, where a particular level is considered as a point on a continuous line. One of the earliest of such attempts was the Whittier scale, devised by Williams (**61**), who was interested in a measure of the social level of the home which would have meaning for his studies of criminal delinquency. He used 5 categories—presence of necessities, neatness, size, parental conditions, and parental supervision—each of which was rated on a five-point scale. The reliability of the scale is undetermined and it is, of

[5] Decroly and Degand (**18**), Binet and Simon (**6**), Morlé (**44**), Hoffman (**32**), Weintrob and Weintrob (**56**), and Bridges and Coler (**8**).

course, subject to all the known sources of error involved in the use of rating methods. Several workers have used this scale, particularly Terman and Merrill (54), Fukuda (25), and Burks (12).

Most of the other scales in use depend upon known occupation of father for a criterion of economic status. A good many of the studies [6] are based on a classification suggested by the Census Bureau, using a scale ranging from category I, professional and higher executive, to category VII, unskilled labor. Even here, however, there appears to be little uniformity, the categories tending to vary considerably from investigator to investigator. Terman and some of his students have tended to place reliance on the Barr scale (3), which was constructed by having 30 judges rate 100 occupations for intelligence on a scale from 0 to 100.[7] A few of these studies use Sim's scale,[8] but Terman and Merrill (54) report a much lower reliability for it than was originally claimed. Finally, a number of investigators (Freeman, Leahy, Bayley, Burks, Stroud, and others) have constructed their own scales and questionnaires.

When we note the wide variety of means utilized to gain a measure of socioeconomic status, we feel constrained to indicate the probability that few of these studies were measuring the same thing in the same way. And where such differences exist, it is difficult to see how we may interpret the obtained differences in intelligence test score. On the other hand, it must be recognized that, whatever the method used for measuring socioeconomic status, all these studies agree in finding a tendency for IQ to grade off with social level. In the absence of a really precise and reliable instrument for measuring social status, this writer believes that an occupational hierarchy, of the type suggested by the U. S. Census classification, would afford the most objective means of indicating social level. Although it is

[6] Representative here are those of Collins (17), Goodenough (28), Haggerty and Nash (30), Dexter (19), Book (7), Terman and Merrill (54), Duff and Thomson (20), and Russell (46).

[7] It should be obvious that the position of an occupation on this scale represents nothing more than the pooled judgment of 30 men as to the amount of intelligence necessary for success at a particular occupation. In the absence of any really adequate criteria as to the intelligence associated with each occupation, these judgments can be little more than an expression of the *stereotypes* of these judges. One would like to know more of their social origin and social outlook than is reported by Terman.

[8] Furfey and Muhlenbein (27), Chapman and Wiggins (15), Terman and Merrill (54).

admitted that there must generally be considerable variation in cultural level within a single occupational category, the danger of the sources of error involved in all rating methods would be avoided if occupation were taken as the sole criterion.

The application of continuous scales for measuring socioeconomic status is, of course, a desirable thing where correlational techniques are to be utilized, and this need has been at the basis of most of the scales constructed. At the same time, the variation in precision of the scales used may account, in part, for the lack in agreement of reported correlations (cf. Table II). On the whole, the use of correlations is more dangerous in this field than in many others. If full precautions are taken to obtain a representative sampling, then the distribution of socioeconomic status will be skewed toward the upper end of the distribution, a conclusion which is confirmed by several of the studies under consideration. When it is recognized that variable degrees of skewness in distributions to be correlated affect the size of the coefficient of correlation to a degree not precisely known (the common measures of variability and central tendency have little meaning for skewed distributions), one realizes that one is dealing here with a possible misuse of statistical procedures. It is, on the whole, wiser, for these reasons, to eliminate correlational techniques from our methodology in this particular field, and to restrict ourselves to presentation of our data in distributional form.

In this section we have pointed to the very wide variation in the methods used to measure socioeconomic status, recognizing that precision of measurement here is as vital as precision in the case of the measurement of intelligence. With regard to the most sophisticated measures used—those produced by the use of graduated scales of social level—we have indicated certain weaknesses in their structure which render results obtained in their use indecisive. At the present stage of development of these scales, we feel that a more reliable and more 'objective' method of measuring social level is afforded by some such occupational hierarchy as is employed by the U. S. Bureau of the Census, provided that all workers agree on the meaning of the terms used. Finally, we have raised certain doubts concerning the proper use of correlational techniques in this field, particularly where, by the very nature of the case, distributions of social level tend to be skewed, thus rendering obtained correlation coefficients unfit for precise interpretation.

Analysis of the Facts

We are now in a position to set the obtained relationship between socioeconomic status and intelligence within the more inclusive framework of all that is known about 'intelligence' and 'intelligence testing.' In so doing, we shall find it necessary to engage in a reanalysis of many of the basic questions raised in this field in order to discover their implications for the problem under consideration. First, we shall raise certain questions concerning the constancy of the IQ; secondly, we shall study the measurable degree to which certain environmental variables may affect intelligence test score; and finally, we shall consider certain fundamental points with reference to the basic assumptions involved in the entire field of mental testing.

The Constancy of the IQ. The question of the constancy of the IQ brings with it, of course, the problem of the reliability of the standard tests of intelligence. The point here may be simply stated. How much may IQ be expected to change with time, both on the average and for individual cases? It is clear that the critical variable here is the length of the time interval that occurs between test and retest. There are a number of studies in this field, most of which have periodically been reviewed by Foran (**21, 22**). Two techniques have commonly been used. The first involves correlating a given group of individuals for test-retest scores; the second proceeds by making a detailed distribution of obtained differences in IQ upon retest and studying the characteristics of the distribution. By the first method we learn the degree to which individuals maintain their relative standing within the group tested, disregarding the magnitude of gains or losses; by the second procedure we gain some insight into the degree of individual variation in actual score.

So far as the first method of studying constancy is concerned, Brown (**10**) presents interesting results. He points out that previous investigators found correlations ranging from +.85 to +.95, where the test-retest interval was under a year. Realizing that the use of longer intervals might have critical meaning for the question of constancy, he reports data for intervals ranging from a few months to 12 years. His results may conveniently be summarized by stating that intervals ranging from 1 to 24 months yielded a combined r of +.81, but intervals ranging from 60 to 145 months yielded an r of +.61. The fact which emerges from this study is that the predictive efficiency of a test-retest r for the Stanford-Binet drops from

55% better than chance where the time interval is relatively small (below a year) to only 20% better than chance where the time interval is larger (from 5 to 12 years).[9]

The importance of this result is enhanced if we study the relationship from the point of view of the distribution of individual changes. According to Terman and Merrill (**54**) and others, the *average* test-retest change in IQ is about five points, when the group tested is fairly representative of the general population, and where the test-retest interval is small. The *mean* change, however, is not as significant for our purposes as is the range of the distribution. Where data have been reported in this latter form,[10] we are able to point to considerable variation within the group tested. If we disregard the direction of change, we find that the *range* of change in IQ points, even when the test-retest interval is small, appears never to be *less* than 20 points. Hildreth (**31**), in one of the most elaborate studies of constancy we possess, reports a range of 50 points; A. W. Brown (**9**) and R. R. Brown (**10**), both working with problem children but using time intervals of different lengths, report ranges of over thirty points. The latter author points to a considerable increase in both the total and interquartile range of change when the time interval is increased to over five years.[11]

These data indicate a point of major importance which has tended to be glossed over in most studies of constancy. All of these studies

[9] R. L. Thorndike (**55**) has been able to arrange r's found in the literature (he did not have Brown's study) in descending order of size with increase in the length of the test-retest interval and fit a linear equation to the scatter of plotted points. From this equation he predicts an r of $+.69$ at an interval of 60 months, a conclusion which accords well with Brown's finding.

[10] It is a sad commentary on the misuse and misunderstanding of statistical procedures that even the most experienced workers in this field tend to present their results in terms of means or medians and neglect to describe the actual distribution. The main interest is *not* in measures of central tendency, which tend to mask individual variation, but in what happens at the extremes of the distribution. This tendency to exploit means has brought with it the serious consequences of a tendency to transform a mean number into a limiting number. So, in a recent book written for popular digestion, we find Cattell (**14**, p. 30) citing the general result of a mean change of 5 points on test-retest and concluding, "I would predict that the most extreme changes of environment in the community will prove to change IQ by 3 to 4 points—at the utmost 5 points—and then only when the influences are effective in the very early years."

[11] In contrast to a mean change of about five points, attested to by many investigators where the test-retest interval is small, R. R. Brown reports a mean change of 10.7 for his entire group and one of 12.7 points for his 60- to 145-month interval.

report individual cases of change in IQ from test to retest of over twenty points and, where the interval is long, individual cases of over fifty-point differences may be observed. It should be a fair question to ask what factors were operating to produce these enormous changes in given individuals. We have no right to assume that environment carries out its rôle uniformly with respect to all members of a group. If this assumption were fact we should be at a loss to account for this considerable intragroup variation in the constancy of the IQ. On the contrary, we should feel more justified in hypothesizing that those individuals who show large changes from test to retest are precisely the ones whose educational and social history should have been teased out in the greatest detail. Unfortunately, the studies of constancy under consideration in this section of our paper make no attempt even to raise the problem. It is the writer's opinion that until we know more of the events which lead up to these large changes in IQ—changes which might bring an individual from the 'moron' level to 'normal,' or from 'normal' to 'very superior'—we are not justified in assuming that the IQ is "relatively constant," which is the conclusion of most of the workers in this field. Rather, we might discover that IQ is relatively constant just so long as the relation between the individual and his total environment remains relatively the same, but that IQ undergoes change with marked shift in the total situation. Direct proof of this hypothesis is wanting since investigators have tended generally to study group changes rather than individual changes, but we shall attempt to gather indirect proof by an examination of extant studies on the relation of environment and intelligence.

The Effect of Residence in a Poor Environment. It is unfortunately true of studies of the constancy of the IQ that no attempt has ever been made to analyze the obtained results in terms of socioeconomic status as a variable. The subjects of these studies, as indicated above, have not been dealt with as individuals whose social background it would be important to know, but have been looked upon as the sources of numbers yielding means and sigmas. Although every competent clinician is aware of the fact that an obtained difference in IQ must be imbedded in the social background of the person before it can sensibly be interpreted, such cautions do not appear generally to have been followed in the research literature.

Some light may be shed on the conditions which produce variation in the IQ score from test to retest, if we study the factors operating at the extremes of environmental variation. It is true

that we are still dealing here with groups rather than individuals, but at least we are concerned with entire populations, living more or less as units on different levels of socioeconomic status. One of the most interesting studies of this type is that of Hugh Gordon (29), who examined the characteristics of English children at the lowest extreme of social and cultural level. One of his groups was made up of "canal-boat children," an extremely isolated group who live with their families on the freight boats that ply the canals of England. He discusses their social characteristics in some detail, pointing out that the average school attendance of these children is about 4 or 5% of the total time during which urban children of equivalent social class attend, that they have very little social contact with other children on land, and are regarded, to a considerable degree, as outcasts and pariahs by the land population. He obtained a sample of 76 children (estimated total in 1919 was about 1000 children), for whom he reports a mean IQ of 69.6 on the 1916 Stanford Revision. More interesting is the relation between age (length of residence in the environment) and IQ. He obtains an r of $-.76$ between age and size of IQ, which may be expressed more graphically in the following table where familial relationships are analyzed (Table III):

TABLE III

THE RELATION BETWEEN AGE AND IQ FOR CANAL BOAT CHILDREN

Group	N	Mean CA (Years—Months)	Mean IQ
Youngest in Family.....	5	5–10	90
Third " "	11	7– 4	77
Second " "	22	8–10	73
Oldest " "	22	11–11	60

The number of cases in any single category is, of course, too small to permit any final generalization, but the trend is extremely clear, lending support to the rather high negative correlation which Gordon obtains for his entire group of which these cases form a part.

Gordon also reports data on a group of gypsy children, whose schooling amounts to about 30% of the norm and who do not live quite so isolated an existence as the canal-boaters. These gypsy children show a mean spread with age from IQ 94 to 73, reflecting a correlation between age and intelligence of —.43. While generally somewhat indecisive because of the small number of cases studied, these data of Gordon's indicate the degree to which the Stanford-Binet may be affected by environment, in this case not by *changed*

environment, but by continued existence at a critically low level of social and cultural status.

Very recently a comparable study has appeared in this country. Asher (**2**) administered 4 standard intelligence tests (the Myers Mental Test, the National Intelligence Test, and the Herring and Stanford Revisions of the Binet) to a large group of Kentucky mountain children. As is well known, the East Kentucky mountaineers live an extremely isolated existence at a very low cultural and economic level, perhaps lower than that of any other rural group in the country, and certainly far below urban groups. The median IQ for a group of over three hundred children on the Myers test was 67.5, on the N. I. T. for 250 cases it was 71.5, and on the Binet for 56 cases it was 72.9. Like Gordon, Asher analyzes the data in terms of age, gaining a notion of the effect of continued residence (Table IV).

TABLE IV

AGE AND IQ FOR KENTUCKY MOUNTAIN CHILDREN

Age (Years)	N	Mean IQ
7	25	84
8	29	76
9	47	74
10	51	69
11	49	67
12	52	64
13	43	63
14	28	63
15	15	60

In connection with these significant data, Asher observes that "tests constructed and standardized in one locality for and on particular groups . . . should not be used in some other locality or on some other group . . ." (**2**, p. 485).

An interesting converse to the studies of Asher and Gordon is that of Klineberg (**36**). Attempting to throw light upon the question of selective migration as a possible explanatory factor which would account for the test superiority of northern over southern Negroes, he obtained a large sample of negro boys who were alike in chronological age but differed in the length of their residence in New York City, all having migrated from the South. He finds that those who have lived in New York less than one year obtain a mean Binet IQ of 81 and that there is a steady rise with length of residence up to 88.5 at a three- to four-year residence, with little change thereafter. The same general result is obtained with a group of girls

tested with the N. I. T., with an even more marked spread being disclosed. A further check removed the possibility of a deterioration among the more recent migrants.

An additional study of the effects of continued residence in a poor environment is that of Jordan (**34**), who tested all public school children of a North Carolina mill town found in Grades I to VII. He finds the usual type of correlation with occupation and comments on the considerable degree of skewness of the distribution for the entire group. More important for us is his incidental finding that the median score of millworkers' children decreases from about IQ 100 at the age of 6 to about IQ 85 at age 13. Like Gordon and Asher, Jordan ascribes these results to the fact that the tests used (Pintner-Cunningham, Dearborn, and N. I. T.) were standardized on groups enjoying superior environments.

Although we shall consider orphan- and foster-child studies in some detail in the next section of this article, there are 2 such studies which present data relevant to the present problem. In connection with a large-scale investigation of British orphan children, Laurence (**40**) reports that correlations between the social status of parents and IQ of their children appear to increase in size the longer the child has lived with the parents. Concretely, children who entered an orphan home before they were 3 years of age show correlations with social level of true parents of about +.20, while children who entered the institution at an age above 3 yield a correlation of about +.45. The number of cases in the 2 groups (68 and 107, respectively) is too small and sampling errors too uncontrolled for us to accept this data as decisive.

Skeels and Fillmore (**51**), in this country, working with larger groups and under better controlled conditions, present more conclusive data. They were able to obtain Stanford-Binet IQ's on 407 children shortly after commitment to the Iowa Soldiers' Orphan Home. These children made up 132 families of 2 or more children each, ranging in age from 1 to 14 years, thus making it possible to study trends in IQ with increasing residence with the parents. It should be mentioned that these children came from generally underprivileged homes, 67.6% of their fathers being day laborers, with no father enjoying a status higher than that signified by occupational category III of the U. S. Census classification. In general, Skeels and Fillmore find a fairly steady decline in IQ with increasing age on entrance, the drop being from approximately IQ 110 at chronological age 1 to IQ 79 at age 14. The same result is obtained more strikingly

when they divide their group into paired siblings and study the effect of the size of the age differences between the members of a pair. These differences, always in favor of the younger child, range from 3 points at a one-year difference to 15 points in IQ at the nine-year difference. Critical ratios of these differences range from 2.1 to 3.1, showing fair reliability considering the necessarily small size of the separate groups. If we divide the entire group into only 2 categories, comprising those pairs separated by differences smaller than 3 years as opposed to those with a difference greater than 3 years, the critical ratio of the differences rises to well above 3.

These 6 studies give us significant data on the degree to which IQ may be depressed by continued residence in environments offering poor cultural and educational opportunities. Gordon and Asher show that a drop of 30 points may be expected where the social level is lowest, whereas Jordan's and Skeels' data indicate expected declines of 15 or 20 points where the environmental extreme is not so great. Klineberg shows that a change from a poor to a better environment will increase IQ. Laurence's results are not easy to interpret, couched as they are in the form of correlation ratios, but she appears to find the same trend. In general, we may conclude from this section of our study that 20 to 30 points in IQ may be lost, *on the average,* by continued exposure to a low environment. Individual loss may, of course, be greater, and one should not be surprised to discover declines as great as 50 or 60 points in IQ from year 1 to year 14 in some cases.

Foster Child Studies (The Effects of Environment). The main generally available life situation which makes it possible to study the effects of translation from one social level to another is that involving the adoption of orphan children. For this reason, a number of investigators have carried on extensive researches in this field.[12] The value of these studies has varied, however, with the degree to which the respective researchers have recognized the importance of certain variables. Such matters as age on adoption, length of time in foster home, social level of foster home, IQ level on entrance, education and culture of foster parents, all appear to be influential on the amount of change observed, not to mention such less well-defined and more qualitative factors as the possible difference in attitude of a foster parent as compared with a real parent.

[12] The important papers are those of Freeman, Holzinger, and Mitchell (23), Burks (12), Leahy (41), Skeels (49, 50), and Skeels and Fillmore (51).

The most elaborate of the early studies are those of Freeman and Burks, which together form a large section of the epochal *27th Yearbook of the National Society for the Study of Education.* It should be realized that both of these researches were part of an effort to settle the problem of nature and nurture, and were not specifically directed toward our problem. At the same time they present data which have important implications for the problem of socioeconomic status. Burks was interested in the degree of correlation obtaining between the IQ's of foster children and various intellectual and social criteria of the foster parents. She studied over two hundred foster children who were legally adopted before the age of 12 months, and whose chronological ages ranged from 5 to 14 years at the time of the study. She utilized a control group of about one hundred children, matched with the foster group for age, sex, and the neighborhood, occupational status, and nationality of the foster parents. For measuring intelligence she used the Stanford-Binet, and applied the Whittier scale and a "culture-rating" of her own to measure social status. Her main result is that adopted children correlate with the social and mental level of foster parents to a degree of about +.25, whereas her control children correlate with their true parents to a degree of approximately +.45. On the basis of this finding she concludes that "close to 75 or 80% of IQ variance is due to innate and heritable causes" (12, p. 304).

We are, however, more interested in another aspect of her study. She reports that the mean IQ for her foster group was 107.4 and points to the fact that, on the basis of the known social background of the true parents, the mean should be around 100. Since selective placement is not a likely explanation (correlations between social status of true and foster parents all range around zero), she attributes this apparent mean gain of 7 points to the effect of environment. Quite startling in this respect was her finding that the correlation between the social status of the true parents and the IQ of their children, who have been living with foster parents for some years, *was +0.07 at the time the study was conducted.* Finding that the mental age of her foster parent group averages about one sigma above the theoretical mean, she infers that measurable environment of one sigma above the mean does not shift IQ of foster children by more than 6 to 9 points above the value it would have had under a normal environment. Assuming normality of the distribution of social status (a belief that is sociologically unjustifiable), she con-

cludes that the " maximal contribution of the best home environment is apparently about 20 IQ points . . . conversely, the least cultured, least stimulating kind of American home environment may depress IQ as much as 20 points." In our opinion, this conclusion of Burks' has great significance for our problem, showing as it does that a shift from a very poor to a very good environment may bring a change in IQ of the order of 40 points.

Freeman, Holzinger, and Mitchell (23) administered the Stanford-Binet to a very large number of foster children, who could be grouped in various ways for analysis. A defect of this study is that most of the children were placed rather old (mean age of placement in foster home was 4 years). Nevertheless some of their results are interesting. A group of children (average placement, age 8 years), who were tested before placement and retested on an average of 4 years after placement, showed a mean gain of 2.5 IQ points, which, when corrected for the artificial 'top' of the Stanford-Binet, rose to 7.5 points. It should be emphasized that the general level of the foster homes for this group was not high. Those placed in the better homes gained 5 points (uncorrected), while those placed in the poorer homes showed no change. In addition, age was a factor, those being over 12 years on placement showing no gain, whereas those below 12 gained an average of 6 points. Their study also reports some suggestive correlations between separated siblings and between foster and own children—correlations which may valuably be compared to those found generally in the literature between siblings reared under ordinary conditions. They find an r of +.25 between orphan siblings at a mean age of 5 years reared in different foster homes; the older the sibs at the time of separation the higher is this r (it rises to +.43 for those separated at ages over 6). They further obtain an r of +.38 between foster and own children reared in the same home (N=47), one of +.40 for foster siblings reared in the same home (N=93), and a very startling r of +.34 between *unrelated* siblings reared in the same foster home. Positive relationships are found with age on entering, length of stay, economic level of foster home, and so on. Finally, they obtained a correlation of +.48 between IQ of orphan children and socioeconomic level of foster home, a figure which rises to +.52 for those children adopted before the age of 2 years. They consider that all these data taken together indicate the very great influence that environmental variables may

have on intelligence test score, a conclusion in which we may support them, with the very serious qualification that their attempts to control selective factors were not always successful.

More recently Leahy (41) has carried on a research in an effort to afford better control of errors of sampling and selective placement. She obtained a large group of adopted children which fulfilled certain rigid requirements: they were placed before the age of 6 months; they were not less than 5 nor more than 14 years of age at the time of the investigation; they were legally adopted by married people; and they were White, non-Jewish, and North European in descent. A control group of own children was matched with the fosters in age, sex, parents' occupation, education, and nationality. Leahy constructed her own test for socioeconomic status, and used the Stanford-Binet and Otis Intermediate A for testing the children and parents, respectively. In general, she confirms Burks' older results, finding r's of the order of +.25 between foster parents and foster children, as compared with r's of from +.45 to +.50 for true parents and children. There are several interesting considerations raised, however, by a more critical perusal of her data. Since her entire procedure was adopted to avoid selective placement (and with considerable success), we should expect the IQ range of her subjects to be similar to that of the general population, or even perhaps to show a lower mean, since orphanage children tend to come from the lower economic classes. Yet we find that she reports a mean IQ for her adopted children of 110.5, with an S.D. of 12.5. Although she tells us nothing of the social background of the real parents of the adopted children, we can infer that this figure represents a very large mean gain in IQ. If we analyze Leahy's subjects according to the occupational status of the foster parents, we obtain the following array (Table V):

TABLE V

IQ of Children According to Occupation of Foster Parents

Occupation	N	Mean IQ	S.D.
I. Professional	43	113	12
II. Semiprofessional	38	112	11
III. Skilled Trades, etc.	44	111	14
IV. Rural Owners	..	...	..
V. Semiskilled, etc.	45	109	12
VI. Slightly Skilled	24	108	14
VII. Unskilled Day Labor	..	...	..

If we compare Table V with Table I, we will note striking differences. The range is not so wide and the grading off in IQ not so extreme in this group as in the case of Terman and Merrill's subjects. We now begin to see some possible explanation for the relatively low correlations between adopted children and their foster parents. The quite different social situation presented in the case of adoption, as compared with that prevailing in an ordinary home, may make for a more intense cultural relationship between the foster parents and the child. We see that even children adopted by occupational category VI average 108 in IQ, as compared with 98 for Terman's equivalent group (cf. Table I). This indicates the possible operation of some factor or factors tending to keep IQ as high as is possible. The hypothesis we are here suggesting is that foster parents, operating under the knowledge that the children are not their own, may take greater pains in providing the best social and cultural environment that is within their power, thus somewhat weakening the effect of nominal social status as a factor in influencing IQ. It is emphasized that the above is nothing more than hypothesis and demands extensive sociological and psychological investigation. Nevertheless, while this possibility remains, the mere difference in size of correlation coefficients, subject as they are to all sorts of difficulties in interpretation, cannot be taken as conclusive proof.

We shall close this section of our paper with an analysis of a quite recent series of papers by Skeels (**49**, **50**, **51**), which report what is without doubt the best research on foster children. Up to the present time, Skeels has managed to collect intelligence test records and a great deal of sociological data on 147 children adopted before the age of 6 months. He used the Kuhlmann revision of the Binet on the younger children and the Stanford revision on the older. His results are so important for our problem that we shall examine them in some detail. First he tells us of the social background of both true and foster parents, which, as expected, form 2 quite divergent groups. The occupational curve for true parents is skewed upwards whereas that of foster parents is skewed downwards, *i.e.* 57% of true parents and only 4.8% of foster parents fall in the 2 lowest occupational categories, whereas only 2.6% of true and 23.8% of foster parents are found in the 2 highest categories. Similar divergencies are found in the education of parents and IQ of mother, when available. These factors lead Skeels to conclude that selective placement played a

minimal rôle in this situation. Skeels' most startling result may be discovered in the following table (Table VI):

TABLE VI

CORRELATIONS BETWEEN IQ OF CHILD AND VARIOUS FACTORS RELATING TO TRUE AND FOSTER PARENTS

Correlations Between IQ of Child and:	True Parent			Foster Parent		
	N	r	P.E.	N	r	P.E.
Education of Father.....	90	+.05	.07	142	+.10	.06
Education of Mother.....	137	+.12	.06	141	—.05	.06
Midparent Education	88	+.08	.07	139	+.04	.06
IQ of Mother.....	78	+.04	.08	...	...	...

Perusal of these correlations reveals the astounding fact that all of them hover around zero, are far outweighed by their P.E.'s, and indicate complete lack of any consistent relationship. In other words, *there is no resemblance between the characteristics of these foster children and those of either their true or foster parents.* Reasons for this lack of relationship become clear if we study the characteristics of this group of children. First of all, their IQ's are disproportionately high. Their mean IQ is 115.4; only 4% are below the theoretical mean (IQ 100) and none fall below the dull normal level; 41% have IQ's of 120 or above. Briefly, the mean IQ and the general characteristics of the distribution are comparable to those which other investigators have reported for the children of occupational categories I and II. On the other hand, the median occupational level of the *true* parents falls at category VI (slightly skilled labor) and of the *foster* parents at category III (clerical and skilled trades). On the basis of the occupational status of the true parents, we should expect a mean IQ for the children of about 98, and on the basis of that for the foster parents we should anticipate a mean of about 108, both being much lower than the obtained mean.

Skeels believes that 2 factors may play a part in determining this result, although he admits some lack of positive evidence. First, he points out that the foster parents represent a selection upwards within each of their occupational categories, for the very good reason that the orphanages in question were very careful where they placed their children. Secondly, he raises the same question that we raised with reference to Leahy's study, that foster parents are obviously interested in having a child in the home and may take greater pains to encourage the intellectual development of their foster children. Whatever factors play a part in the foster child situation, it is obvious that these children benefit greatly by adoption. Skeels' group pre-

sents us with a probable mean gain of 17 IQ points, a gain which was necessarily even greater for given individuals of the group.

One additional point should be made before we leave Skeels' studies. We have already indicated the existence of a zero correlation between true mothers' IQ and children's IQ. It might be even more conclusive if we present the data in detail (Table VII).

TABLE VII

THE RELATION BETWEEN IQ OF ADOPTED CHILD AND IQ OF TRUE MOTHER

IQ Level of True Mother	N	Mean IQ of True Mother	Mean IQ of Children
Above 100	14	105.3	117.6
90–99	23	94.3	116.2
80–89	19	84.9	116.8
70–79	13	75.2	119.7
Below 80	22	69.6	116.5
Below 70	9	61.6	112.0
All Cases	78	87.0	116.7

It may be seen that the general average of these mothers is well below 90, and that the entire distribution is apparently positively skewed, yet the children who have never lived with their true mothers average at 117. Very disconcerting to those who believe in the rigid inheritance of feeble-mindedness should be the 9 cases of mothers with IQ's below 70 (mean=61.6) whose children average at 112.0.

Age and Socioeconomic Status (The Effects of Environment). It is clearly important to know how early in life occupational differences in intelligence manifest themselves. If, from the first few months of life, children are found to grade off in intelligence according to the social level of their parents, it would be extremely difficult to account for these differences in environmental terms. On the other hand, if this relation to socioeconomic status was not present at first and only began to develop as the child grew older, we should have first-class evidence of extrabiological conditions at work.

The evidence here is conflicting. Goodenough (**28**), and Terman and Merrill (**54**), among others using variations of the Binet, find that occupational hierarchies appear as early as the two-year level. Below this point they do not go, the tests being considered unreliable when used on children younger than 2 years of age. On the other hand, Furfey (**26**), and Bayley (**4**), using "babytests" of the Bühler variety, find zero correlations between the mental ability of infants and socioeconomic status of parents when the children are below the age of 18 months. Bayley, particularly, presents a long table of correlations between midparent education (education and occupation

correlate +.78 for her group) and the child's point score which range about zero until the age of 18 months, and then begin to climb rapidly, reaching an r of +.47 at 36 months. The number of cases at each of her age levels is too small (about fifty) to yield clear reliability, but the trend of the data is clear.

The point at issue between the researches of Goodenough and Terman and those of Furfey and Bayley will not be clear until we discuss the whole question of the standardization of tests of intelligence, but some statements are appropriate here. The tests of early intelligence, of the type devised by Bühler (11), Linfert and Hierholzer (42), and Bayley (4), have been seriously attacked on the basis of the allegation that they may not be measuring the same abilities as those measured by the standard intelligence tests. A perusal of these early tests indicates that they are rather heavily weighted with items differentiating postural and motor abilities (reaching, crawling, perceiving, etc.) as compared with the verbal and intellectual performances exploited by tests of the Binet type. More serious evidence of a lack of relation between the early and later tests is afforded by a little study of Furfey and Muhlenbein (27), who retested with the Stanford-Binet 71 children originally tested with the Linfert-Hierholzer scale 4 years before. They report a zero correlation between relative score on the 2 tests. We must tentatively conclude that we are dealing here with 2 quite different situations. At the same time, we are not at liberty to say precisely what factors play the significant rôle in these 2 types of tests. If the Binet-type test is heavily weighted with verbal factors, as most experts agree, then it should not be surprising that a different spread arises where preverbal performances are at a premium. This may signify that what these investigators mean by the term 'intelligence' varies with the situation—an implication which has importance for a functional view of intelligence. At any rate, we must regard the question of the precise character of the relationship between age, intelligence, and socioeconomic status as unsettled at present, with the belief that Bayley's results deserve very careful follow-up.

Identical Twins (The Effects of Environment). For some years, psychologists interested in the nature-nurture controversy have turned to studies of familial resemblance, particularly to examinations of resemblances and differences among twins. This line of attack upon an old problem has much to recommend it from the point of view of scientific method, but it is rather complicated and quite subject to sampling errors since the groups studied are small.

The most decisive evidence in this field, of course, is afforded by studies of identical twins, especially where individual pairs have been found who have been reared apart since birth. It is to be regretted that most of these studies have paid little attention to considerations of differing socioeconomic status, and thus lose considerably in value for our purposes. It should be quite obvious, although apparently it has not been so, that one cannot speak of individuals as being reared in differing environments merely because they were brought up in different homes. Intrapair twin differences can have real meaning only when a detailed description of respective home background is supplied. The only study with which we are familiar which makes an effort to attempt such a description is a very recent research carried on jointly by Freeman, Holzinger, and Newman (**24**). They have managed to collect all sorts of data on 50 pairs of fraternal twins, 50 pairs of identical twins reared together, and 19 pairs of identical twins reared separately since shortly after birth. It is really only with the latter group that we are seriously concerned. These workers used the Stanford-Binet and the Otis test to measure intelligence and an environmental rating scale of their own construction to measure socioeconomic status. In addition, they applied an achievement test (Stanford) and the Woodworth P-D sheet as well as other tests. They also obtained numerous physical criteria of differences and similarity. Their general finding may be expressed in the following table of correlations (Table VIII):

TABLE VIII

CORRELATIONS BETWEEN 3 GROUPS OF TWIN PAIRS

Criterion	Identical Twins Reared Together (N=50)	Fraternal Twins Reared Together (N=50)	Identical Twins Reared Apart (N=19)
Standing Height	.981	.934	.969
Body Weight	.973	.900	.886
Stanford-Binet IQ	.910	.640	.670
Otis IQ	.922	.621	.727

Perusal of Table VIII will yield the information that identical twins reared apart resemble each other no more than fraternal twins reared together. More important for our purposes is Freeman, Holzinger, and Newman's analysis of this result in terms of social background. First of all, they make the very emphatic point that the differences between the "environmental ratings" of the separated pairs were large only in a few cases. Examination of Table 87 of their monograph (**24**, p. 335), combined with perusal of the detailed

case studies they submit, reveals that in no case was a twin pair placed at the 2 extremes of the distribution of social status, *i.e.* one member at category I, the other at category VII. The largest socio-economic difference (only 1 case) is between II and VI. The following table shows the relationship between certain social criteria and the extent of intrapair difference in IQ (Table IX):

TABLE IX

CORRELATION OF INTRAPAIR DIFFERENCE IN IQ EXISTING BETWEEN SEPARATED IDENTICAL TWINS AND ESTIMATED DIFFERENCES IN ENVIRONMENTAL BACKGROUND

Criterion	Education (years of formal schooling)	Social Status (rating scale)
Binet IQ	*+.791**	*+.507*
Otis IQ	*+.547*	*+.533*
Stanford Achievement	*+.908*	+.349
Body Weight	+.095	+.226

* Italicized numbers indicate statistically significant correlations.

Intrapair differences in IQ range from 1 point to 24 points, with a mean difference for this group of 8.2. All in all, this study indicates clearly the considerable effect that social status may have on level of IQ. The authors conclude that "extreme differences in educational and social environments are accompanied by significant changes in intelligence and educational achievements as measured by our tests" (24, p. 349).

DISCUSSION

When Binet and Simon began their epoch-making work, some thirty-odd years ago, the problem facing them was apparently quite simple. The French Minister of Public Instruction had asked them to find some means of distinguishing between lack of mental ability on the one hand, and laziness or lack of interest on the other, in so far as these 2 groups of factors contributed to poor work in school. This was their problem, and the history of mental testing since their early work indicates that they dealt with it fairly satisfactorily. On the other hand, various other problems have arisen in this field for which the tests were not precisely devised, and which have led to a vast amount of controversy and conflicting interpretation. Most of these problems revolve about the basic question of the amount of IQ variance due to innate ability, as opposed to that due to education and life experience. Our own examination of the fundamental character of the relation between social status and intelligence test score necessarily involves this basic question.

In order to be clear on the points involved, we must engage in an analysis of the assumptions at the basis of this whole field. So far as the writer can determine, these assumptions may be limited to 3. First, it is assumed that mental ability increases both in efficiency and scope with increasing chronological age up to a certain point, and then tends to remain constant or level off. Secondly, it is assumed that the amount of education and information a person manages to gather is somehow a direct function of the level of his innate ability. Thirdly, it is assumed that mental ability is distributed in the population according to the Gaussian 'normal' curve. It is with these assumptions, combined with certain subsidiary implications to which they lead, that practically all investigators have approached the tasks of the construction, application, and interpretation of the standard test of intelligence.

Let us see how these assumptions have worked out in practice in the construction of these tests. If we examine the generally accepted procedure in the make-up of a test of intelligence, we note 2 or 3 significant steps. First, the investigator gathers together a large number of mental tasks, varying in apparent difficulty from some that would be easy for an elementary school child to others that would strain the abilities of a college graduate. The researcher then gains access to a number of persons, his *standardization group*, to whom he applies these mental tasks. Those tasks, then, which differentiate the group with respect to some accepted criteria—age, scholastic advancement, teachers' estimate of ability, and so on—are retained and those which do not satisfactorily differentiate the group are eliminated. New tests are added if necessary, and are checked in the same way. Finally, the investigator comes to the end of his task with a battery of tests of mental ability of known difficulty, which he may then arrange in a manner which is most convenient for his purposes.

It should be realized that this procedure is *empirical* from first to last, and that the final test which emerges is by its very nature a *relative*, rather than an absolute, instrument of measurement. Interpretation of a given score on a test made up in this manner *has meaning only with reference to the characteristics of the persons who made up the standardization group in the first instance.* This conclusion should be obvious from a consideration of the procedures outlined above, and is the only one consistent with logic and scientific method.

It should now be clear that precise knowledge of the individuals making up the standardization group is *all-important* for a correct and scientifically justified interpretation of an intelligence test score. The writer finds it all the more reprehensible, then, that generalizations have frequently been made in the field of mental testing without taking this basic fact into account. Not very long ago, men were using tests standardized on native-born, American, white children to measure the mental ability of groups as diverse in culture and background as Mexican immigrants, American Indians, and American Negroes. When these groups obtained lower mean scores than native-born American whites, investigators jumped to hasty conclusions with reference to the relative inferiority and superiority of different racial and national groups. In the field of 'racial intelligence,' Klineberg (37) particularly has shown why these conclusions were hasty, and most authorities are now agreed that a test standardized on one racial or national group cannot be applied to a group of differing culture and background.

Now, if the importance of the standardization group is admitted where we are dealing with differing national or racial groups, what relevance has this fact for our purposes here? It will be remembered that most intelligence tests measure education and information to a considerable degree, on the assumption mentioned earlier that the more intelligent an individual is, the more knowledge and skill he will be able to absorb or acquire.[13] This may generally be admitted to be true, but the assumption leads to a dangerous fallacy when social factors cut across, as they necessarily must. If every single individual in society had precisely equal opportunity, encouragement, and stimulation to acquire education and information, and if the fund of things to be learned was common for all, then it might follow that intelligence test score would be a fairly accurate measure of ability.

[13] Kelley (35) has estimated that 90% of a standard intelligence test and a standard educational achievement test measures the same abilities over a school grade range from fourth to eighth grade. Decisive evidence of the degree to which education and cultural training are factors in intelligence test score is afforded by Wellman's very remarkable series of papers (57, 58, 59, 60). Her most important results comprise conclusive demonstrations that attendance at a preschool clinic is not only influential in considerably raising IQ (individual cases are reported of gains of as much as 70 points), but also that these gains occurred only during the time the child spent in the clinic and did not take place at all during the time the child spent at home. As a very important addition, it should be mentioned that these very large gains were permanent; that is, the children tended to maintain the level which they had reached at the end of the preschool period.

But examination of the structure and make-up of intelligence tests convinces one that all that is measured here is the mean performance of a standardization group, which thereafter functions at the level below or above which inferiority and superiority is to be fixed. If social status has anything to do with opportunity for education, cultural stimulation, and intellectual attitude, and the writer does not feel that an extensive sociological analysis is necessary to show that it has, then it is not at all surprising that social status and intelligence test score are positively correlated. Even if one takes pains to bring together a standardization group which really represents a cross section of society (a feat never achieved by anyone), it is still not clear what the results will imply. If our earlier statements as to the relation between social status and cultural background are true—and they are supported by facts which cannot be argued away—then the standardization group *itself* will level off with social status in proficiency at many tasks (vocabulary, information, knowledge of relationships, verbal proficiency, learned skills, etc.), a result which will be permanently self-perpetuating as soon as the test is applied in practice.

That the standard intelligence tests generally have attempted to use as the norm the child of 'average social status' may be seen by examination of those better known. In his 1916 revision of the Binet, the test used most widely in our studies of social status and intelligence, Terman apparently made an effort to procure a large number of children of middle status. He tells us that "The schools selected for the tests were such as almost anyone would classify as middle-class," and that these were found in communities of average social status. In addition, no children were foreign-born, few were of other than Western European descent, and all were white. On the basis of about five hundred children, whose social status was estimated in each case by the teacher, we find that those of 'average' social status obtained a mean IQ of 99.5, near enough 100 to be considered perfectly at norm. In his latest revision, Terman made a fairly successful attempt to gather a standardization sample which was really representative of the social levels of our society, but here again the norms tell us only of mean proficiency at the time of construction of the tests and yield no information upon the causes for the relationship. Kuhlmann's revision of the Binet (**39**), used by Goodenough in her widely quoted study of social status, was based on subjects from county fairs, orphan homes, and public schools, and we cannot even guess at their social background.

It is important to see precisely where these considerations lead. If we summarize the important points of our evidence very briefly we see that (1) the standardized tests are far from 'constant,' particularly where the test-retest interval is large; (2) continued residence in a very poor environment brings about a considerable loss in IQ, often from the 'normal' level to that of 'dull' or even 'borderline'; (3) translation from a poor to a good environment brings about large increases in IQ which, at the extremes, may amount to 30 or 40 points; (4) there is a strong possibility that the positive relation between age and social status does not exist below a certain age level; (5) some evidence from identical twin studies indicates that there is a correlation of about +.50 between social status and intelligence even where heredity is held constant; (6) the inequality of social and economic opportunity renders suspect one of the major assumptions basic to the construction of the tests; namely, that knowledge and information are a direct function of native ability, and that the former may be used to measure the latter.

All these points taken together lead to a conclusion which we feel is forced and inescapable. Just as Klineberg has shown that the standard intelligence tests are inadequate instruments for measuring the native ability of different races, *so do we find that these tests cannot be used for measuring the capacity of different social levels within our own society.* It should be emphasized that we are not raising here the question as to whether all of the abilities measured by the tests are determined exclusively by environment, although it should be clear from the studies analyzed above that environmental variables are very important. Rather, we are raising the question as to whether the generally obtained relationship between social status and intelligence test score requires any other explanation than an environmental one. We feel that there is strong, perhaps decisive, evidence for a negative answer. In other words, we have tried to show that although individuals at birth may differ in native endowment (a still incompletely settled question), it has definitely *not* been proved that social status of the parent has anything to do with the native endowment of the infant. That a positive relationship later in the life of the individual may develop is hardly denied. But all the summarized studies tend to show that low cultural environment tends to *depress* IQ approximately to the degree agreed to as characteristic of laborers' children, and that a high environment *raises* IQ correspondingly. All, then, of the twenty-point mean difference in IQ

found to exist between children of the lowest and highest status may be accounted for entirely in environmental terms.

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THE EDUCATIONAL VALUE OF "DRIVERS' CLINICS"

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About a dozen mobile "drivers' clinics" are now operating in the United States. They are sponsored severally by insurance companies, automobile associations, or educational institutions. The sponsors of some of them (**1**, **3**, **5**, **9**, **13**) assert that their main purpose is "educational." They therefore urge that the clinical staff must be specially trained and must include a skilled psychotechnologist who can interpret the results to the subjects.

These clinics are often set up at automobile shows, at state fairs, or in police departments, and are well advertised by some local organization interested in safety. Each subject is tested for various characteristics of his vision, reaction time, estimation of speeds of visible objects, accuracy of steering a symbolic vehicle, etc., and is then allowed to see his score. Ordinarily the procedure is so planned that he is almost certain to make a low score in at least one test. This, according to DeSilva (**1**), has an advantage:

> "Assembling many tests to form a clinic permits us to find a weakness in everyone. Once a clinic supervisor pierces the armor of conceit of Mr. Average Driver, he can often get him to break down and talk about himself. Demonstrating a relevant defect . . . will jolt the confidence of any individual and open the way to self-study and self-discovery."

The director of the clinic may then offer the subject some specific advice, which presupposes that, if he is deficient in any skill demanded by any of the tests, he is subject to a corresponding special hazard against which he ought to take special precautions. Thus, according to DeSilva (**2**):

> "If the driver has a slower than average reaction-time, he is told to drive less rapidly and follow less closely after the car in front of him. Or if he has a narrow field of vision on one side, he is advised to turn his head more frequently in that direction; if he is more sensitive to glare than the average person, he is advised to keep off the roads as much as possible at night. . . ."

It has been questioned whether the skills demanded in these tests are also demanded in driving, or whether deficiencies in them are signs of a lack of driving skill. To answer the question, one must find out whether or not unselected individuals whose test scores are subnormal had abnormally high accident rates preceding their clinical advice. The present authors are now investigating this question, but are not yet ready to report their findings.

Meanwhile, certain persons insist that the social utility of advice is independent of its factual and logical basis. They ask, rather, "*Does it work?* Does it cause the drivers to reduce their accident rates, or not?" For a long time no evidence was published which purported to show that this test of social utility had been satisfied. But recently DeSilva published the following assertion (**2**):

> "About a year ago approximately 500 accident (making) drivers were tested and interviewed in one State department. A subsequent check on their accidents since the examination reveals that they have gone a whole year with only one-seventh as many accidents as they had in the preceding year. Without a doubt, repeater drivers can be improved and numbers of them completely salvaged."

It is not perfectly clear from this quotation, taken alone, that the author offered this finding as proof that the advice was beneficial, and other articles (**4**, **5**) are even more noncommittal. But E. L. Yordan (**14**), then director of publications of the Harvard Bureau for Street Traffic Research, apparently referring to the same facts, calls his recital "the dramatic story of how 500 accident-repeaters were cured to the extent that only three had mishaps after 3,250,000 miles of driving by the group"; says that the 500 drivers were selected by the registrar of motor vehicles of Massachusetts "as having the worst accident-records in the commonwealth"; and asserts that during 8 months after they had been tested and advised they collectively had but 3 accidents, while another group of 500 repeaters, which had a better record than this group had before the tests, but who were not tested and advised, had, collectively, 20 accidents in 8 months. He calls it "a success story unparalleled in automotive history . . . a story with a great lesson for the future; a story that should bring hope to all who despair of becoming better drivers, of breaking their accident jinx."[1]

[1] What we shall presently say about interpretations of this set of findings applies also to another set, recently reported by the Personnel Research Federation (**11**). The report seems to be based on some reputed findings of C. S. Slocombe, as a special adviser to the Division of Motor Vehicles of the Com-

One may indeed grant that *if* the advice had been beneficial, it would have been followed by a reduction in the accident rates of the subjects who received it. One may grant also that these rates actually declined as reported. But one cannot conclude that *because* the rates so declined, *therefore* the advice was beneficial. This form of reasoning is "affirming the consequent"; it is utterly fallacious; and, being insidious, it often escapes detection, as it has so escaped many readers in this instance.

We shall now undertake to show that if one studies a class of operators who are selected because they have had an abnormally high accident rate in one moderately short period, one will find that they will very probably show a lower accident rate in the following period of similar length, whether they are subjected to special treatment in the meantime or not, and even though the treatment that they may receive is wholly ineffective. Conversely, if one studies a class of operators who are selected after they are treated, and because they have had an abnormally high rate in a fairly short period following the treatment, one will discover that they will show a lower accident rate in a similar period preceding the treatment, even though the treatment may be wholly ineffective. Hence, one can draw no conclusion as to the effect of the treatment from knowledge of the behavior of a single selected class, for the outcome of the test is partly and perhaps wholly determined by the rule of selection of cases.

For example, Connecticut requires reports of all accidents occurring on the highways which involve death or injury to any person or property damage exceeding $25. Massachusetts requires every operator to carry insurance against personal accidents and to report to the Registry of Motor Vehicles every accident which involves personal injuries, however trivial. In these states, which have unusually efficient, though still imperfect, systems of detecting and reporting automobile accidents, the average number of accidents per operator per annum is low, being 0.0399 in Connecticut during the period 1931–1936, and 0.0368 in Massachusetts during part of the same period. Thus, in either of these states the rate is roughly one

monwealth of Massachusetts. According to this article, 1000 operators were selected because they had been charged (collectively) with 1893 accidents in the preceding 6 months, and sent to the adviser for a conference. In this conference the circumstances of their individual accidents were reviewed, and they were given advice based upon the diagnosis of the counsellor. In the next 6 months they were collectively charged with only 16 accidents. It remains to be determined whether this relatively huge improvement was probably due to the advice, rather than being an accident of the method of sampling.

accident per operator in 25 to 27 years (12). Moreover, the Pearsonian coefficient of correlation between the rates of 29,531 Connecticut operators licensed throughout 2 successive triennial periods is low, being of the order of 0.2 for the periods 1931–1933 and 1934–1936. Thus, if one applied the Pearsonian regression equation to the whole population, to deduce the rate of each operator in one period from knowledge of his rate in the other, one's estimate would be subject to a standard error about 0.98 times as large as the standard error attending the guess that each individual operator had the average rate of the sample population. Finally, in all such populations that we have examined, the frequency distributions are limited in both directions. It is impossible that any operator should have an annual rate less than zero; and it is very improbable that any operator should have a rate greater than some value which is readily ascertained (7, 12).

Thus, the conditions which DeSilva describes resemble the hypothetical condition which Jung (8) recently proposed, in which 10 imaginary subjects were to be ranked by dealing to them 10 shuffled cards, numbered from 1 to 10, and "treated" after the first deal by the laying on of hands. Under such conditions it is impossible that in the next deal the subjects who made the lowest set of scores should change their status as a class except by gaining; or that the subjects who made the highest set of scores should change their class status except by losing.

In this situation, if an operator in Connecticut or Massachusetts is charged with one or more accidents in a year, his rate for that year is at least 25 to 27 times as high as the rate for the whole population, and (due to the form of the distribution) the difference between his rate and that of the whole population is many times the standard error of the difference. It is, therefore, highly improbable that he and the other operators having the same rate as his, or still higher rates than his, could change their mean rates as a class, except by lowering them. On the other hand, it is impossible that the accident rates of those operators who have gone accident-free for one period should change in the next period of the same length, except by increasing.

In general, any class of operators, selected according to their accident rates in one period will tend in a similar period to "regress toward the average" of the population from which they were drawn. If these operators are subjected to special treatment between the 2 periods, the treatment may either retard this "centripetal drift" (8), accelerate it, or allow it to proceed undisturbed. Only experience

can determine the probable effect on this tendency, and the strength of the tendency must first be ascertained. This condition cannot be satisfied from examination of a single selected class; it requires, as a minimum, a four-fold classification and census of the entire sample population.

For illustration, we choose a sample population consisting of 29,531 general drivers of Connecticut, each being licensed throughout the period 1931–1936. One of the present writers (J) was responsible for the plan of the census and the investigation (12) based upon it, and for the presentation and interpretation of the results. From the alphabetical card index of some 408,000 operators who were relicensed in 1932, and who were therefore licensed in 1931, the investigators drew every tenth card. From the selected list which they so compiled, they eliminated all operators except those whom they could identify from the records of the commissioner of motor vehicles as having been licensed in every year of the period 1931–1936. This period may be regarded as 2 consecutive triennia; the first triennium being 1931–1933, the second triennium being 1934–1936. From this sample population we have formed 14 classes, designated below, each class including all those (and only those) members of the sample population who satisfied the rule according to which the class was formed. The classes were as follows:

TABLE I

Class	Basic Triennium	Number of Accidents Charged to Each Operator
1	First	3 or more
2	Second	3 or more
3	First	Exactly 3
4	Second	Exactly 3
5	First	2 or more
6	Second	2 or more
7	First	Exactly 2
8	Second	Exactly 2
9	First	1 or more
10	Second	1 or more
11	First	Exactly 1
12	Second	Exactly 1
13	First	None
14	Second	None

Thus, the odd-numbered classes were selected according to their accident records in the first triennium, and the even-numbered classes according to their accident records in the second triennium. Table II compares the performances of each of these selected classes in the 2 triennia. Classes 1, 3, 5, 7, 9, 11, and 14 each showed more

operators involved, more accidents, and a higher group-rate in the first triennium than in the second; the other classes show the reverse. The last column of the table shows that in no instance can the change be plausibly attributed to chance, for in the most favorable instance, Class 4, the probability that such a difference as was obtained would occur by mere chance is of the order of $(10)^{-32}$, or about one in a hundred thousand billion billion billion.[2]

TABLE II

ACCIDENT-RECORDS IN 1931–1933 AND 1934–1936 OF 29,531 GENERAL DRIVERS OF CONNECTICUT CLASSIFIED ACCORDING TO THEIR PERFORMANCE IN ONE OR THE OTHER TRIENNIUM

A	B	C	D	E	F	G	H
Class	Number of Operators	Number of Operators Charged in First Triennium	Number of Operators Charged in Second Triennium	Diff. (D–C)	Number of Accidents Charged in First Triennium	Number of Accidents Charged in Second Triennium	Diff. (G–F)
1	41	41	14	—27	133	22	—111
2	40	21	40	19	34	126	92
3	31	31	9	—22	93	15	—78
4	34	17	34	17	29	102	73
5	398	398	96	—302	847	129	—718
6	353	92	353	261	121	752	631
7	357	357	82	—275	714	107	—607
8	313	71	313	242	87	626	539
9	3272	3272	584	—2688	3721	701	—3020
10	2962	584	2962	2378	699	3361	2662
11	2874	2874	488	—2386	2874	572	—2302
12	2609	492	2609	2117	578	2609	2031
13	26259	0	2378	2378	0	2660	2660
14	26569	2688	0	—2688	3022	0	—3022

I	J	K	L	M
Class	Ratio G to F	Average Number of Accidents Charged per Operator in First Triennium	Average Number of Accidents Charged per Operator in Second Triennium	Diff. (K–J)	Probability that Difference Would Occur by Chance
1	1 to 6.1	3.2439±0.0452	0.5366±0.0934	—2.7073±0.1037	$(10)^{-69}$
2	3.7 to 1	0.8500±0.1081	3.1500±0.0381	2.3000±0.1146	$(10)^{-41}$
3	1 to 6.2	3.0000±0.0000	0.4839±0.1060	—2.5161±0.1060	$(10)^{-57}$
4	3.5 to 1	0.8529±0.1227	3.0000±0.0000	2.1471±0.1227	$(10)^{-32}$
5	1 to 6.6	2.1281±0.0136	0.3241±0.0224	—1.8040±0.0262	$(10)^{-469}$
6	6.2 to 1	0.3428±0.0235	2.1303±0.0138	1.7875±0.0273	$(10)^{-427}$
7	1 to 6.7	2.0000±0.0000	0.2997±0.0224	—1.7003±0.0224	$(10)^{-569}$
8	7.2 to 1	0.2780±0.0214	2.0000±0.0000	1.7220±0.0214	$(10)^{-640}$
9	1 to 5.3	1.1372±0.0047	0.2142±0.0060	—0.9230±0.0076	$(10)^{-1467}$
10	4.8 to 1	0.2360±0.0065	1.1347±0.0048	0.8987±0.0081	$(10)^{-1222}$
11	1 to 5.0	1.0000±0.0000	0.1990±0.0060	—0.8010±0.0060	$(10)^{-1743}$
12	4.5 to 1	0.2215±0.0066	1.0000±0.0000	0.7785±0.0066	$(10)^{-1368}$
13	.....	0.0000±0.0000	0.1013±0.0014	0.1013±0.0014	$(10)^{-514}$
14	.....	0.1137±0.0015	0.0000±0.0000	—0.1137±0.0015	$(10)^{-582}$

[2] In each instance the probability was computed from the approximate formula given by Pearson (**10**, p. 11), $P=2F$, this approximation being in our opinion adequate for the special instances in which the probability is in any event "very small."

Although the accident makers of the first triennium, forming Classes 1, 3, 5, 7, 9, and 11, definitely improved in the second triennium, while Class 13, consisting of those operators who went accident-free in the first triennium, definitely became worse, nevertheless Class 13 had only about half as many accidents per operator in the second triennium as the best of the other classes and less than one-fifth as many as Class 1 had in the second period. Thus, the "centripetal drift," though clearly evident, is opposed by a tendency for each class to maintain its relative status.

Let us now examine 4 classes: namely, 7, 8, 13, and 14, and suppose that each of their members had been given some quite ineffective clinical advice at the end of the first triennium. If we considered only Class 7, we should notice that in the second triennium only 82 of its members (instead of 357) were involved in accidents—a saving of 77% of the operators; that in the second triennium only 107 accidents were charged against its members (instead of 714); that its accident rate in the second triennium was only 1/6.7 of its rate in the first triennium, this ratio approaching DeSilva's ratio of 1/7 within the probable error of the determination; and that the probability of the difference being due to chance is only $(10)^{-569}$. On hasty examination this looks as if the imaginary treatment had been effective, and the rule of reasoning employed by Slocombe, Yordan, and possibly by DeSilva appears to yield that conclusion. Classes 1, 3, 5, 9, 11, and 14 stand in a similar situation.

Suppose, however, that we should consider Class 8. In the second triennium, 313 of its members were involved (instead of 71). This makes 4.41 times the ratio of involvements holding for the first triennium. Also, 626 accidents were charged against its members in the second triennium (instead of 87). Thus, its accidents were 7.2 times as numerous as in the first triennium—almost exactly the inverse of the corresponding ratio for Class 7, and the probability that the difference between its rates for the 2 triennia would have occurred by chance is only $(10)^{-640}$. If the rule of reasoning presupposed by these authors were valid, it would require us to conclude that the imaginary treatment was harmful. Classes 2, 4, 6, 10, 12, and 13 stand in a similar situation.

To make an adequate appraisal, one has to divide the sample population into 2 classes, one of which is to be advised and the other not. Each of these classes has then to be divided into at least two, according to their accident histories before the advice was given. In the simplest possible instance, 4 classes would result: (a) accident

makers who were advised; (b) accident makers who were not advised; (c) accident avoiders who were advised; (d) accident avoiders who were not advised. It is important that classes (a) and (b) should be homogeneous with respect to their previous accident histories, and that (c) and (d) should be homogeneous with respect to their previous histories.

If all the rules of sampling that are necessary to permit comparison have been satisfied within a suitable standard of tolerance, then we may consider the consequences of several hypotheses:

(1) The advice was generally beneficial. This implies that although both classes (a) and (b) may have improved in the second period, nevertheless, class (a) improved the more; and that although both classes (c) and (d) may have deteriorated in the second period, nevertheless, class (d) deteriorated the more; and that neither of these differences is plausibly attributable to chance.

(2) The advice was generally detrimental. This implies that class (a) improved less than class (b), while class (c) deteriorated more than class (d), and that the differences are not plausibly attributable to chance.

(3) The advice was generally ineffective. This implies that there is no statistically reliable difference between the changes in behavior of classes (a) and (b), or of classes (c) and (d).

(4) The advice was irregularly effective, in the sense of being beneficial or detrimental for one segment of the population, and either ineffective or effective in the opposite sense for other segments. This implies that the results should be inconsistent among certain of these classes, but still reliable.

Hypothesis (4) is especially worth entertaining, for there is no good *a priori* reason for expecting the advice to be uniformly effective upon all classes of operators. Certain kinds of musical instruction, for example, seem to make poor singers sing better and good singers sing worse. Likewise, certain kinds of medication seem to help those who are suffering from certain specific diseases, and injure those who are well or who may suffer from certain other diseases. So, also, it is conceivable that advice which shakes the conceit of the unobservant and inept might interfere with the already adequate habits of those apt drivers not previously addicted to morbid introspection.

In a word, the findings reported by DeSilva and his colleagues, and also by Slocombe, *et al.*, concerning the behavior of a single selected class, prove nothing. They are equally consistent with the

hypothesis that the treatment was without effect upon that class and with the hypothesis that the treatment was beneficial. If they have some valid evidence, let us hope that they will present it in such a form that one may be able to evaluate it.

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BOOK REVIEWS

STERN, WILLIAM, *General Psychology from the Personalistic Standpoint.* (Trans. by H. D. Spoerl.) New York: The Macmillan Company, 1938. Pp. xxii+589.

Stern's *Allgemeine Psychologie auf Personalistischer Grundlage* is here presented in an excellent English translation by Howard Davis Spoerl. Professor Spoerl has done his work well. The result of his painstaking care and sympathetic understanding is an eminently readable translation that distorts neither the meaning nor the expression of the original. The labor of Gordon W. Allport in assisting both the author and translator in making available in its present form this vital contribution to current psychological thought should also be mentioned.

The book surveys the field of psychology from the viewpoint of Personalism, the core of which is the concept of the *person,* "a living whole, individual, unique, striving toward goals, self-contained and yet open to the world around him," and insists that the findings of psychology can be organized only by reference to such a unique, living individual as the originator, carrier, and regulator of all psychological states and processes. The personalistic approach, and hence the book, can be characterized by two main tendencies. The first of these is the stress laid upon the uniqueness of the individual. The second is the reorientation of the material of psychology around an experiencing individual as a systematic focal point. It is in these two directions that Personalism makes its major contribution, and the book may be evaluated accordingly.

The first characteristic, the stress upon the uniqueness of the individual, is critical rather than constructive. We are warned that laws based upon group uniformities may fail upon application to the specific individual. The plaint is an old one and involves one of the fundamental difficulties of science, that scientific laws are based upon general tendencies although our interest in predicting and understanding is more often in one particular case. We incorporate this warning under such concepts as probable error, standard deviation, etc., but having taken note of the danger there is nothing left for us to do but proceed as before. The issue has recently been raised by Lewin and by G. W. Allport, both of whom make keen, critical use

of the uniqueness of the individual but fail in developing a constructive program around this point. We are left with a clear vision of the fallibility of statements based upon group uniformities, but are given no constructive substitute for the *collective* methods of contemporary psychology. So, in Stern we find the caution that intelligence test scores have significance only as group statistics and imply nothing about the individual, and yet he finds it necessary in talking constructively about the individual to use just such generalizations from group data as the intelligence tests personify. This is not to deny the force of Stern's criticism nor the necessity for its continual reiteration, but merely to point out its purely destructive nature. For a constructive formulation of psychology we must go to that second aspect of Personalism in which the individual is used as a reference point for the integration of all psychological process.

Through this stress upon the individual as the functional center of psychology Stern's book achieves a brilliant and stimulating organization that is distinctly biological, developmental, and functional, and is very close in spirit to the practical tendency that has been so typical of American psychology. It is popularization in the best sense, in that the findings of psychology are taken from their impersonal context and related to the adaptive strivings of a living organism.

The first section of the book contains a clear and interesting discussion of psychology as a science, and lays the systematic basis for the later treatment of specific subjects. The second part, on sensation, is the best in the book. Here the classical and modern experimental findings are endowed with new biological and social significance and there is systematic integration without neglect of the established work in the field. The following section on memory continues in this fashion, its keynote being memory as the history of the person and his w orld as it is experienced. One might object to the overuse of the concept of mneme or to a few general statements about memory that lack Stern's usual critical acumen, but these are minor details in view of the excellence of the whole. From this point on, as the book progresses through sections on thought and imagination, striving, acting, and achieving, and a final section on feeling, there is less and less reference to the experimental findings of psychology and more and more reliance upon logical reasoning. The book becomes less empirical and more rational, and loses somewhat in doing so. One might say that it becomes less universal and more personal. One misses the eclectic scope of the first half of the

book. It may well be that the treatment of thought, will, and feeling lends itself to a logical rather than an experimental approach, but there are places where a wider reference to experimental results would have strengthened the systematic structure. Thus, the section on learning is seriously damaged by the scanty attention paid to contemporary animal psychology, and a discussion of insight and generalization would have benefited tremendously by reference to the work on irradiation of conditioning and equivalence of stimuli. Nor does the merely rational treatment of feeling completely satisfy the reader. There are parts, however, that retain the thoroughness of the earlier sections; and, even when the argument is most philosophical, it is brilliant and always stimulating.

To say that the book is essentially rational and logical rather than empirical, that in this way it is more "German" than "American," is not critical condemnation. One might wish for more anchoring of terms to experimental findings. One might wish that the references to American work were not so largely limited to Stern's personal friends. One might wish that he were less given to the somewhat unworthy device of refuting his opponents by giving his own arbitrary definition to their terms. These are minor considerations. "General Psychology" remains a fine book. It is "systematic" psychology at its best—an orderly, serious presentation of the subject that does justice to psychology by reviewing its content—and yet it manages a stimulating integration of its own. Despite its definitely systematic approach, it is a remarkably eclectic treatment. This last suggests that although Personalism may not be a conclusive system for psychology it is nevertheless calling attention to some rather basic truths to which contemporary psychologists could well pay more attention.

It is not a textbook for the average introductory class, but it could be used in advanced classes. Despite its seriousness, it is interestingly and humanly written. Much of its illustrative material is of the homely, understandable sort that lightens and popularizes the more abstract passages. Certainly, no psychologist could fail to read it with pleasure and with profit. It is particularly gratifying to read a serious general psychology that is written interestingly and intelligibly, without creating the impression that the author's attention was fixated upon his publisher's idea of a textbook that would sell. "General Psychology" is an important and worthwhile book.

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SPEARMAN, C., *Psychology Down the Ages.* (Two Vols.) London: Macmillan and Company, Ltd., 1937. Pp. xi+454 and vii+355.

Were one seeking evidence for the thesis that history is a function of the historian, one would find it in the histories of psychology, with this book as an excellent example. Making no effort to show the development of psychology in temporal sequence, Spearman paints the intellectual background of those aspects of modern psychology in which he is interested and closes the second volume with chapters which are primarily on his own work. He proposes "to outline how far scientific psychology has really made progress" (I, p. 6), to do this without exaggeration, and in particular to see in what ways it has gone beyond common sense. The book does not compete with other histories; it is a history, "but of a peculiar kind" (I, p. 6). It will "deal mainly with the breaking of new ground" (I, p. 6), but will aim "to draw up a fair and square balance-sheet" (I, p. 7).

The new ground is not experimentalists' ground, but is that of English theorists and statistical psychologists. The first volume is written in the tradition of the histories which present psychology as a branch of philosophy, but departs from that tradition in frequent gestures toward the results of experiment. The second volume is in the tradition of English theory and modern statistical psychology. A student who did not know contemporary psychology would derive from either little notion of the experimental subject it has become. Experiment is in the background of emphasis; it is there, but it does not stand out with any force. When Spearman describes "Psychological Methods: Psychology As It Is" (I, Chap. IV) experiment is said to be important, but it is coördinate with hypothesis, analysis and synthesis, and mathematics, and they are not shown as powerless without good experimental method.

Throughout, the book is oriented toward the past on the one hand and toward Spearman's laws, with his work on "G" and the S's, on the other. It shows only in certain limited respects "how far scientific psychology has really made progress." The temporal sweep varies. He sometimes brings a notion, or his citations about it, close to the present, but often he does not. He sometimes refers to experimental work on a problem, but often he does not. Through both volumes, as a unifying thread, runs the drift toward and the arrival at the ideas which have preoccupied their author.

The avowed search for the extent to which psychology has gone beyond common sense recurrently appears. Spearman is quick to applaud an advance beyond common sense, but he is also quick to

show a return to it; one suspects sometimes that he delights in showing the return as much as he delights in the advance. Some of the cited returns to common sense or to the doctrines of the ancients are open to question. In a sense he is usually right when he avers that a supposedly modern idea has been known since early times, but, in another, he reads too much into earlier theorists. Even when they used the words which we now use about a problem they meant them differently. Especially different are the meanings when ours are based on experiment as theirs were not. The rise of experimental attack on the events called 'psychological' has brought with it a wealth of new and relatively precise meanings, based, even though sometimes remotely, on experimental operations. These meanings are different from those of Aristotle or the philosophers of the Middle Ages when they used the same words.

The book shows historical learning, especially in theoretical doctrines enunciated by men trained in philosophy or theology. Not only are the better known men and doctrines discussed, but also many others, especially Oriental and Arabian, referred to in no other history of psychology than Brett's, and not always there. It is written in the style we have learned to expect from Spearman: learned, with many neatly turned phrases; here and there a faint flavor of condescension, a benign confidence in his own ideas. Scholarliness pervades it.

The forty-two chapters are grouped in five parts: "What Psychology is About," "What the Psyche Can Do," "How the Psyche Is Constituted," "What Follows What," and "What Goes with What." The first volume is studded with quotations, the references to most of which (though not completely to all) are given in the notes at the end of Volume II. This copiousness of quotation, although promoting accuracy, is sometimes clumsy. The second volume, being a history of Spearman's own and related work, is more direct exposition and freer from quotation.

The first section proceeds from definition, in which Spearman states his preference for the word "psyche" and its compounds to denote the data of psychology, through chapters on varied relations between mind and body and on the relations of philosophy and psychology, to a concluding chapter on method. Psychology is said to be handicapped by a wrangling over philosophical issues whose solutions are indeterminate, yet "in any effective mental science, these same topics are to a certain extent indispensable" (I, p. 76); without them psychology would be "ridiculous." The way out is for

experimental psychology to take the problems which are open to "its comparatively light methods of attack," leaving the presumably more fundamental problems to "the heavier guns of philosophy" (I, p. 77). Though he believes that some of the problems of philosophy can probably best be treated by departing from experimental results, his reliance on the results of the laboratory is not overweening. The chapter on method, previously referred to, is one indication of this.

The second section on "What the Psyche Can Do" examines some of the concepts of psychology. Of the subject's two great scientific problems, what exists and what coexists, the concepts here dealt with belong to the first. From philosophical notions of intellect he passes to the modern one of "intelligence," "to which probably the greatest amount of labour has been devoted" (I, p. 130), the one which has aroused "the greatest enthusiasm" (I, p. 131). Admitting that quantification of relative amounts of labor and of enthusiasm is not possible, the reviewer doubts that the category of intelligence has elicited more of either on the part of serious investigators than have several other problems, such as sensation or learning. However that may be, Spearman regards intelligence, as defined by the testers, to be equivocal because there is no stable definition upon which all agree. An operational concept thereof does not concern him.

Chapters on attention, other faculties of knowledge, notably memory and imagination, and orectic faculties follow, with a chapter on "Faculties or Chaos" concluding the section. The prevalent accusation that faculties are "superfluous mannikins" (I, p. 189) or else devoid of meaning is unfounded. The word "faculty" is a translation of *facultas* which was an abbreviation of *facilitas*, meaning "bare facility" (I, p. 188), the possibility of the activity in question. The meaning of activity or power did not accrue to it until modern times and then illegitimately, in the light of its history. The further criticism that a "faculty" implies events functioning as a unit in which the parts are mutually dependent, as in the mental discipline case where training of a faculty in one way supposedly trains it equally or nearly equally in other ways, is beside the point, since the historical faculty psychologists did not mean this. The moderns who decry faculties assume such concomitant functioning under other names such as "ability" or "capacity." Discard concomitant function, 'goingness-together,' and the result is intellectual anarchy. Spearman uses the word "faculty," but his quest is for the constitution of mind and the ways in which the constituents go together.

The third section, which is long and acutely written, comes to a climax in the chapter on "The Confusion That Is Gestalt Psychology,"—not that this chapter is the goal of the section but that much of the critical writing in the preceding chapters prepares the way for this one. Much earlier Spearman asserts that the gestaltists' insistence on the artificiality of analysis into parts is very old; and that in this book he proceeds neither from wholes nor from parts but, as nearly everyone does, "from something that is manifestly intermediate" (I, p. 99). In this section his treatment of perceiving relations is especially good. It brings him to the conclusion that psychologists have not regarded perception as a sum of elements but have taken into account the form of the whole, together with acts of discrimination and comparison. They have neglected, however, perception of relations. For Spearman, form is constituted by relations. The two refer to the same facts but stand for different ways of perceiving them.

His critique of *gestalt-psychologie* is interesting and one may hope that the gestalt psychologists will read it. We may pass over the question of whether the gestaltists' doctrines are as old as Spearman asserts them to be, and come to the criticism which he holds most powerful. This is that gestaltists have often derived their concepts from subjective configurations and wrongly applied them to objective ones. From this have come propositions so vague that they may mean anything one wishes to read into them. The outcome is that "if Associationism may be regarded as Psychological Enemy No. 1, cannot Gestaltism put in a claim to be at any rate No. 2?" (I, p. 446).

At the end of the volume the author opines that many readers may be surprised that its tone has been "far less optimistic than most" (I, p. 453). Instead of the usual picture of continual progress, he sees it as "one of discovering, forgetting, and rediscovering. The amiable historians overlook the forgetfulness, and so arrive at unbroken discovery" (I, p. 453). This refers to the fundamental principles with which the first volume deals, not to the admittedly continual discovery of new observations of detail. It may be questioned whether the concepts of the present day are as much rediscoveries as he holds them to be. The words may be the same, but experimental operations have given many of them different meanings. They are, to that extent, new.

The second volume contains less of the past. There are swift backward glances, but the major concern is with laws and statistical discoveries long since connected with Spearman's name. The volume

departs from James' well-known characterization of psychology as "a string of raw facts" having no laws and in ignorance even of the terms between which elementary laws would obtain. The terms, says Spearman, "must somehow be derivable from the mental constitution which we have just been considering" (II, p. 3), though he does not explicitly state them. Thereupon, he examines the nature of scientific law in a significant chapter. A scientific law permits prediction of the future and retrovision of the past, but certain conditions must exist if a law is to be formulated. The course of events must be uniform and must yield to analysis, and the laws formulated should be as general and ultimate as possible. Rational laws are to be preferred, though they may be empirical if this is inescapable. These are the conditions which his summary states as the most important.

The chapter deals with other conditions which the summary omits. There is constancy of events, which Sigwart held is not attained in mental events since they are constantly changing. This objection goes too far in Spearman's view, since the same reasoning would make a science of physiology impossible. There are also the conditions that events must be repeated and must follow one another with temporal continuity; but mental events are not repeated and our knowledge of temporal continuity is doubtful. These difficulties are neither fully discussed nor adequately met. The author makes in this chapter the important point, often unrecognized, that "all exact law is merely ideal. Actual experience can show nothing but approximations" (II, p. 18). This is true in physical science as well as in psychology.

The first candidate for a scientific law is that of association, which is said to represent a large advance over common sense and to give both prevision and retrovision of "important *general* facts" (II, p. 37, italics author's), but to fall short in that it can be resolved into other laws and is therefore not ultimate. This brings us to the two into which it resolves: the laws of dispositions and inertia. The law of dispositions asserts that *"the occurrence of any mental process makes this easier to occur afterwards"* (II, p. 40, italics author's), when "easier" may mean requirement of a smaller amount of the stimulus or an increase in the rapidity, intensity, definiteness, or persistence of the events. The law of inertia states that *"every mental process lags behind the stimulus in beginning and much more so in stopping"* (II, pp. 68–69, italics author's). Both of these laws

are said to meet the requirements of a scientific law and to be a large advance beyond common sense.

The laws of control, constant output, and fatigue, together with the two already stated laws of retentivity "only state how mental processes may undergo increase or decrease of quantity. They fail to indicate what these processes are" (II, p. 119). This is not the place to examine each of these laws critically, but one may at least ask how quantitative some of them are. The author later admits (II, p. 156) that the law of dispositions is silent on the conditions under which it varies from individual to individual or from time to time within an individual. Nowhere does he make a systematic attempt to state the conditions under which the laws yield specific quantitative results. True, the law of basal conditions asserts that "*every manifestation of the preceding four quantitative principles is superposed upon, as its ultimate basis, certain primordial but variable individual potencies*" (II, p. 167, italics author's), but this does not carry us far toward quantitative statement. These laws, as well as those of cognitive and orectic quality, "require to be supplemented by an indefinite number of more specific sublaws" (II, p. 169), and, given adequate sublaws, the "quantitative" laws may become sufficiently quantitative.

The laws of cognitive and orectic quality are already well known, especially the three laws of noegenesis: awareness of experience, law of relations, and eduction of correlates. The noegenetic theory has been largely ignored until recently, but there is said to be a growing number of enthusiastic supporters until "in the last few years it has advanced to the stage of constituting the very foundation of psychological study" (II, p. 170). This may be true in certain quarters, but one notes that the names Spearman cites as explicit supporters are English psychologists and educationists whose writings have influenced Americans, at least, very little. The two orectic laws of impulse and will are said to be orectic and new but as yet of uncertain scientific status.

The concluding section, "What Goes with What," departs from the generality of the preceding four sections, and asks wherein individuals differ. Theories of temperaments and types have failed to show what goes with what, but this question has been answered by "the wonderful device of correlational coefficients" (II, p. 215). In the earlier days of work on individual differences these coefficients had multiplied bewilderingly, were mostly very low, and had no con-

vincing psychological meaning. Then came the factor theory, and especially the factor " G." This theory has the characteristic of distinct modernity. " Whereas the great majority of psychological theorems were already in full bloom thousands of years ago, . . . the Factor Theory seems to have no early precursor whatever, but instead to have come like a bolt from the blue about only a quarter of a century ago " (II, p. 240).

The work on " G," on the specific factors in ability, and to a lesser extent on the orectic factors, is well known and needs no further review. The results, and Spearman's interpretations, are ably summarized in the last seventy-six pages of the book. He pays swift respects to the factorial methods of Thurstone, Hotelling, and Kelley, asserting that these newer methods may be useful for beginning work, but that for definitive conclusions " the older and more potent tetrads " (II, p. 279) are needed.

From what frame of reference shall one evaluate this book? Against the background of the history it calls upon it is erudite and thorough. At the same time it is rigidly selective. The history it deals with is not the history of the laboratory, and the excursions which it makes into the experimental work since 1879 are brief and far from thorough. Of theories, save the author's own, framed within the last fifty years, there is little mention. The author's theories are based, as soon as they depart from immediate touch with the operations of his statistics, on concepts which are not operationally defined. To the reviewer this seems a weakness.

As an outline of " how far scientific psychology has really made progress," the book is incomplete, biased, and misleading. The account of his own work, whether of the laws of general psychology or of individual psychology, is stimulating and important. Much of it is so stated as to set this work in a new and clearer perspective. This is valuable and the book stands as a significant statement of Spearman's work and of his formulation of many of psychology's problems.

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VARNUM, WALTER C., *Psychology in Everyday Life.* New York: McGraw-Hill Book Company, Inc., 1938. Pp. xii+444.

A few years ago, one wrote an elementary textbook of psychology as an exposition of one's systematic position, aiming it at one's professional contemporaries, and allowing undergraduate classes to

stumble through it as best they might. During the last few years a reaction has set in and it has become the fashion to write for the undergraduate. More than one writer has recently shown that this laudable purpose can be achieved without muddled thinking and without loss of accuracy in statement. The volume under consideration in this review, however, has failed to reach its goal by falling prey to the sources of error which beset the worthy path it has attempted to follow. Accuracy of statement has been sacrificed to geniality of presentation and an effort at catholicity has resulted only in a complete lack of coherence in basic structure. The result is a chatty volume which offers the student an insecure and inaccurate hold on certain phases of psychology.

There is much internal evidence to suggest that the fundamental weakness lies in a lack of firsthand knowledge on the part of the author. The documentation of the volume is concerned entirely with secondary and, in some cases, tertiary sources, and the treatment of many topics forces the reader to conclude that the author has encountered only at second hand most of the investigations to which he refers. The suspicion takes root when one encounters repeated references to the work of Johanson on sleep and of Brey on audition; it grows as one finds a perpetuation of the classical error in reporting Stratton's researches in vision, coupled with a misinterpretation of Young's pseudophone experiments; and it bursts into full bloom as one reads that "the frontal lobe is fundamentally motor," that all apparent movement is referred to as *phi*, and that "the level of general intelligence is a factor more important than any other measurable trait for determining the field of vocational activity."

Individually, each of these errors is excusable. Taken together, however, they are significant and are symptomatic of the general level of accuracy. This reviewer, tabulating his comments after careful reading, finds more than a hundred question marks in the margin, each indicating a departure from accurate reporting.

The secondhandedness of the exposition reveals itself in at least two other ways. First, there is a certain uncritical credulity which characterizes the volume as a whole. This leads the author to state that a man is "a well-qualified consultant (for psychotherapy) if he is a member of the A.P.A."; it permits him to report without qualification that "the I.Q. of a person never changes"; and it is apparent in the curious faith which he has that a student may 'settle' for himself some controversial issue by reading about it in secondary sources. Also, there is a lack of care in selecting secondary sources,

which suggests again a lack of direct contact with original materials. The author employs as references some textbooks whose value no one would question, but he lists alongside them certain others whose reception by American psychologists has been by no means unequivocal. There may be other readers who will share this reviewer's hesitation in applauding the author for using Wiggam's *Exploring Your Mind* as a standard reference for most of the chapters.

Inaccuracy in presentation is paralleled by failure to define the basic systematic structure. "Sensation" becomes a veritable shuttlecock which appears now as a unit of perceptual stuff and now as something which "enters the nervous system." The products of the last decade and a half must rest lightly on the shoulders of one who can write, as the psychologist's "technical definition of perception," that "Perception is sensation plus meaning." Nor is the matter improved by the author's failure to understand the context theory of meaning, which he feels constrained to quote at length. We doubt also whether the undergraduate's grasp of the complex nature of motivation will be advanced by his reading that "every effect has *a* cause" (italics the reviewer's). It may be remarked that the expression of thought is paralleled by a literary style which leads the author to split infinitives with monotonous regularity and to grant the sanction of a chapter heading to that remarkable participle "contacting." A minor reflection of carelessness of composition is to be found in the distribution of professional titles. Certain psychologists are presented without benefit of title; others of no greater distinction parade as *Dr.*, *Professor*, and even *Dean*.

The contents of the volume concern themselves with conventional topics in an unconventional arrangement. The reader encounters, first, action and motivation. A chapter on intelligence is followed by a chapter on testing in general, the latter receiving the dubious dignity of the title "Human Engineering." A section on learning leads over into a consideration of effective study methods. "Learning to Use Symbols" introduces speech, reading, and thinking, and includes a simpler etiology of speech defects than most trained readers will be prepared to accept. Succeeding chapters are concerned with motivation and adjustment, with emotions, with the improving of one's personality, and with life-planning. The last two topics prove to be presentations of currently used test methods. Two chapters which share the title "Contacting Our World" afford brief and routine treatments of the senses and the sensory structures. Perception receives summary consideration under the heading "Under-

standing Our World." The volume concludes with a chapter on salesmanship and consumer education which seems to have been put in this particular position because the author could find no other place for it. Approximately half the chapter is based upon a questionnaire study made in an unspecified locality with an unspecified number of respondents.

The reviewer regrets the condemnatory tone of what he has had to write. He is in sympathy with the author's stated objective—a fact which made his approach to the reading of the volume anything but hostile at the outset. If what he has said contains but faint praise for the volume as actually written, it is because of influences inherent in its execution. A closer acquaintance with primary sources may delay the appearance of a revised edition of this work, but it should lead to a marked improvement in the fashion in which the author approaches his praiseworthy objective.

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SPROTT, W. J. H., *General Psychology.* New York: Longmans, Green and Company, 1937. Pp. ix+446.

The fashion of the day is to be eclectic, and for those who admire eclecticism this book should have a certain appeal. Among the almost innumerable textbooks which have appeared during the past few years it stands out as one of the most catholic, one of the most aggressively unbiased, and one of the most solidly factual. It covers almost without exception the standard fields of traditional and contemporary psychology; it presents masses of facts and summarized theories, arranged impressively under topical, numerical, and alphabetical headings; and, except where the author has been unable to resist an aphorism or a *bon mot,* its closely packed pages contain few wasted words. The publisher's notice announces that it was written "for the general reader, as well as for students in Universities, Training Colleges, and elsewhere." The reference to the general reader reveals a degree of optimism which an examination of the text does not warrant. Its usefulness is more likely to be confined to the serious undergraduate who is 'swotting' for an imminent examination.

In the reviewer's opinion this book should not, and will not, be adopted as a textbook for a general course in psychology. What are evidently conceived by the author to be its outstanding merits are really its most glaring faults. To be eclectic is to draw the best from

all available sources, and to integrate the material thus collected into an orderly and meaningful presentation. This has been achieved in the handbooks of many of the sciences, and even in those fields of psychological investigation in which facts can be considered apart from their systematic setting. It is doubtful, however, if such an ideal can be even approached in a treatment of the whole field of psychology. Sprott, at any rate, has signally failed; for, instead of a connected presentation, we have what looks like the expansion of a lecturer's notebook, a series of jottings about various researches and hypotheses, which may be meaningful to the writer but which can be neither stimulating nor instructive for the student. It is, as it were, democracy reduced to an absurdity. Any one fact must be as good as any other. Nothing must be omitted; but nothing must be too vigorously criticized lest its positive contribution be overlooked. In the chapter on visual phenomena, for instance, five of the leading theories of color vision are summarized in less than a page and a half, and the subject is then dropped without critical comment. If the student is to draw his own conclusions, he should be presented with more facts from which to reason. If not, one wonders of what possible use to him such a list of theories can be. Even worse is the following, imbedded in a summary of investigations of thinking: "Flach also investigated the possibility of diagrammatizing abstract ideas" (p. 391). No reference is given, and there is no further discussion of the content of the investigation. Of what value can such a statement be to the reader? The book would have been far better if the author had been less worried about sins of omission.

As a convenient summary of theories and points of view the book may be of some value to the psychologist who lacks ready access to a library. The author has evidently read widely. The classical experiments on thinking, for instance, are ably reported, and the various psychological typologies are listed in detail. Unfortunately, however, the documentation is poor. Initials of authors and dates of publication are not given, and the text contains many references which are not found in the bibliographies.

The author's desire to condense has given a bluntness and choppiness to the style which is at times refreshing. There can be no excuse, however, for a sentence such as the following: ". . . When a person goes out of the room, even when there is no sense-data of them, they are, for a longer or a shorter time, continuing in the present behavioural world in some sense" (p. 190).

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DOUGLAS, O. B., and HOLLAND, B. F., *Fundamentals of Educational Psychology.* New York: The Macmillan Company, 1938. Pp. xv+598.

In reading the first few chapters of this book, the reviewer felt that this text meets the conditions of simple expository style better than any other which has appeared in the field since the publication of Gates' *Psychology for Students of Education.* It later appeared, however, that the beginning student might find some of the passages rather difficult, either because of the abstract nature of the material or because of the somewhat elaborate schematization or classification system employed. Even if this criticism is entirely justifiable, it still must be said that the book is written in excellent prose style.

The book is intended as a first course in educational psychology and, apparently, presupposes no prerequisites in general psychology. Those aspects of general psychology that may be regarded as basic to educational psychology are amply treated in the text. The book is complete as to objectives and range of subject-matter; appropriate educational reference is made; and maturity of thought is reflected throughout the volume. A listing of the chapter headings may serve to give the reader an appreciation of the scope of the book.

Part I. *The Subject Matter and Scientific Bases of Educational Psychology.* Chapters: I, "Educational Psychology and Relative Fields of Knowledge"; II, "The Scientific Standards of Educational Psychology."

Part II. *Environment and Heredity.* Chapters: III, "Nature and Characteristics of Environment and Heredity"; IV, "Bodily Structures and Their Functions"; V, "Growth, Development, and Maturation"; VI, "An Inventory of Inherited Functions and Capacities"; VII, "Characteristics of Original Tendencies."

Part III. *Learning.* Chapters: VIII, "Objectives, Principles, and Types of Learning"; IX, "Sensori-Motor Learning"; X, "Perceptual-Motor Learning: Physiological Tendencies and Social Adjustments"; XI, "Perceptual-Motor Learning: Social Tendencies"; XII, "Perceptual-Motor Learning: Acquired Skills"; XIII, "Perceptual Learning"; XIV, "Conceptual Learning"; XV, "Associative Learning"; XVI, "Imaginative Learning."

Part IV. *Measurement in Educational Psychology.* Chapters: XVII, "Physical, Sensory, and Motor Tests"; XVIII, "The Measurement of Specific Mental Functions and Abilities"; XIX, "Tests of General Mental Ability and Aptitude"; XX, "Educational Measurement"; XXI, "The Statistics of Measurement."

Chapter IV, Part II, treats of the sensory, nervous, and motor

systems in somewhat the same way as they are handled in texts in general psychology, although, necessarily, in a more limited way. The perceptual and thought processes are discussed in Part III in connection with the subject of learning. For example, in Chapter XIII, " Perceptual Learning," the treatment of perception is similar to that found in general psychology with some educational reference to reading and nature study. Chapters X and XI stress emotional stability, conflict, social adjustment, basic motives, and kindred topics. The chapter entitled "Associative Learning" contains the more traditional subject-matter of learning and retention. Of the four major sections of the book, Part IV, *Measurement in Educational Psychology,* appears to be the most satisfactory from the point of view of the student.

The great majority of citations actually made to source material are to the older and better recognized books in the field of educational psychology, such as Gates' *Psychology for Students of Education,* Colvin's *The Learning Process,* and Freeman's *The Psychology of the Common Branches.* However, it is only occasionally, as for example in the treatment of emotion, that the reviewer gains the impression that the treatment is not up to date.

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SHERMAN, MANDEL, *Mental Conflicts and Personality.* New York: Longmans, Green and Company, 1938. Pp. viii+319.

The minor mental deviations of normal people, their maladjustments, neuroses, and delinquencies are the problems of which Sherman here makes his second systematic presentation. The book is admittedly a textbook and not a new contribution to theory and the contents are well indicated by the titles of the seven chapters. These tell of the genesis and nature of conflicts, attitudes, culture conflicts, inferiority and insecurity, sex conflicts, neuroses, and antisocial behavior.

As in the author's previous work, the discussion is fundamentally psychoanalytical, although not consistently Freudian. Concepts such as repression, fixation, distortion, and regression are used without any attempt to interpret them in terms of general and experimental psychology. The keynote of the theoretical approach is clearly indicated by the definition of *conflict:* "A conflict is the pattern of mental activity characterized by unpleasant emotional attitudes involving a discrepancy either between one's desires and the acceptance of social

or other restrictions to their attainment, or between two contradictory or incompatible desires" (p. 2). Conflict, therefore, is viewed not as an objective fact of behavior, but as a "mental" phenomenon. It is a way that an individual feels rather than a way in which he acts.

As is usual to psychoanalytical approaches, the text is substantiated more by references to authority than by the citation of experiments. Of seventy-one references to books and articles given in the footnotes, forty-one cite definitions, classifications, or opinions, while only eleven refer to psychological experiments, ten to sociological studies, five to tests, and four to case histories. The discussion of the problems of "desire" or motivation illustrates this nonexperimental tendency. The author outlines the views of Burt, Freud, McDougall, Thomas, Howard and Patry, Dunlap, Tolman, and Young concerning motivation. Then he gives his own selection of basic motives, but without tracing their nature, origins, or physiological bases. There is no description of experiments dealing with motivation, or of how motives may be learned or modified.

Even more unfortunate is the author's occasional tendency to cite an experiment and then to dismiss it without adequate reason, drawing an opposite conclusion. Thus, Meltzer's study of children's attitudes toward their parents is described at some length (pp. 173–174). But Sherman writes, "The negative findings of Meltzer's study in regard to parental attachments are in marked contrast to the prevailing psychoanalytic theory." He then supports parental "fixations," contrary to the only evidence that he has presented.

Sherman's failure to rely upon experimental evidence is surely not due to any lack of sensitivity or training. He is respected for a number of solid experimental contributions, such as the studies of infants' emotions and the investigations of cultural patterns in the adjustments of the "hollow folk." It is evident that he prefers an explanation based on the intuitive interpretation of case histories to one founded upon precise experimental results. This is a decision the wisdom of which many psychologists will question.

A good feature of *Mental Conflicts and Personality* is the recognition of social and cultural factors in maladjustment, not only in the chapter on "Culture Conflicts," but throughout the book. The sociological material has been well interwoven with the psychological, supplying a need that is often poorly met by other books on this subject.

The new volume differs little in scope or point of view from the author's previous *Mental Hygiene and Education* (1934). The pub-

lisher states that it is intended for courses "on a somewhat more advanced level" than the earlier text. The citation of many opinions on a number of topics is indeed food for the advanced student, and in this respect the book fulfills its aim. But it lacks other requisites of an "advanced" text, especially in its failure to describe the methods underlying its conclusions, and in its very meager bibliographical aids.

LAURANCE F. SHAFFER.

Carnegie Institute of Technology.

FREUD, ANNA, *The Ego and the Mechanisms of Defence.* London: The Hogarth Press and the Institute of Psycho-Analysis, 1937. Pp. x+196.

The intensive theoretical analysis of repression, anxiety, and symptom-formation in early psychoanalytic writings was not paralleled by an equally careful study of other ego functions. Of primary concern was the discovery of the *sources* of conflict rather than the clarification of the mechanics of conflict-resolution. With Sigmund Freud's publication of *The Ego and the Id* and, later, *Inhibition, Symptom and Anxiety,* impetus was given to more detailed examination of the processes by which compromise between incompatible behavior sequences is effected. The present book is a contribution to this problem.

Briefly, threats to the ego are attributed to three sources. The first, objective danger, arouses anxiety, and the ego defends itself by denial in phantasy, words, or acts (undoing), and by restriction of the ego. These mechanisms are familiar enough to readers of earlier psychoanalytic writings but placed in the present context, with case material derived largely from children, they seem to have a new vitality. A second source of danger lies in the internalized conflicts which herald the development of the superego. To meet this kind of threat the ego may employ projection or a technique labeled "identification with the aggressor." The chapter on projection adds little to the theory of that mechanism, but provides an interesting analysis of a form of projected altruism. The third danger is the strength of the instincts themselves, which have the capacity to arouse anxiety in the absence of any learned fear of succumbing to them. It is the author's contention that much of the anxiety and aggression of puberty is a consequence of this threat. This theory has little to commend it, but students of adolescence will, nevertheless, find the final chapters of the book of considerable interest, for they represent

a careful analysis of the ego activities typical of the onset and decline of puberty and exemplify the author's hypothesis that the kind of defence mechanisms used at any given age are characteristic of that stage in the development of the ego. The book does not in any sense present a developmental theory of the ego defence comparable to the Freudian theory of libido development but possibly lays the groundwork for such a contribution in the future.

The major criticism of Anna Freud's approach to ego defences is one which applies to all previous work on mental mechanisms with the exception of Sigmund Freud's classical papers on repression and the unconscious: the definitions of postulated mechanisms are loose and inexact. There is no rigid differentiation between them. Not infrequently bits of behavior could be labeled as exemplifying either of two mechanisms. One reason for this is that words which have two meanings are sometimes used in one sense and sometimes in the other. For example, "identification with the aggressor" means, literally, a special case of aggression in which A's aggression is a response to the frustration he feels as a result of having identified himself with B, *i.e.* B is frustrated, A identifies himself with B, and is therefore frustrated and reacts aggressively. The wrath of the motion-picture audience watching an antiracket propaganda film is a clear case of the mechanism's operation: the audience identifies itself with the frustrated hero and thus has its aggression aroused by the same conditions that frustrate the hero. So long as identification is used to mean the *mechanism* of identification the phrase "identification with the aggressor" is a convenient rubric for indicating this special process. Anna Freud has given at least two case examples (pp. 119–125), however, which are instances of "identification with the aggressor" only if identification means *identical* or similar behavior patterns. The child behaves aggressively toward someone who is behaving aggressively toward him. In neither of these cases is there any evidence of a true identification with the aggressor; it would appear to a reader that one child was aggressing toward a dentist who had injured (frustrated) him and the other was aggressive in response to the frustration of having his previously successful repression of the Œdipus conflict destroyed (with the consequent reactivation of the conflict and anxiety). Still a third meaning of the mechanism (possibly the fundamental one; it is difficult to decide) derives from two cases in which the children attempted, *i.e.* *had the purpose,* to ward off danger by identifying themselves with the dangerous objects—an irritable schoolmaster in one instance and

ghosts in the other (pp. 117–119). Similar confusion exists with reference to the other mechanisms discussed.

Anna Freud demonstrates to an unusual degree the flair, apparently possessed by many psychoanalytic theorists, for crystallizing frighteningly complex behavior patterns into relatively simple operations. It is unfortunate that she has felt it sufficient in her exposition of these crystallizations to present only the denotative data from which the concepts emerge. It is as if she were saying, "Out of all the behavior I have observed, I have chosen these as examples of a special process. You must distill the concept from these percepts. I did." It would have required little additional work to have defined the concepts in unambiguous terms; until that is done, readers will have to rely on their own conceptualizations, which may or may not be the same as the author's.

Another difficulty with the book, paradoxically enough, is that it is too systematic; the arguments rest too directly on the fundamental assumptions of the psychoanalytic system as a whole. That the system in its present form has faults is as apparent to psychoanalysts as to their critics; but that the two groups could agree on the nature of those faults is most improbable. For example, implicit in the present work is the assumption (first made apparent in psychoanalytic theory by Freud's *Psychology of Dreams*) that any given behavioral event may have an essentially unitary motivational source. Several cases are described of children who reacted to intellectual competition by withdrawing from the competitive situation: ". . . when children defend themselves against the 'pain' which they experience on comparing their own performances with those of others, the feeling in question [inferiority, inadequacy, anxiety] is merely substitutive" (p. 104). That is, the objective anxiety induced by unsuccessful competition is not effective as a motivational factor; it is the *remembered* anxiety, resulting perhaps from earlier unsuccessful competition in the Œdipus situation, which instigates behavior. To the experimental psychologist, only too familiar with the complexities of interacting motives in the simplest laboratory situations, this insistence on pointing to single rather than multiple sources of motivation is nothing less than a false assumption.

The consequences of this premise are clearly revealed when it is remembered that the author finds that usually there are different mechanisms responsible for defence against the three different sources of danger to the ego. These sources are essentially three groups of motives. When it is necessary to decide what unitary motive is

responsible for a given anxiety, a decision is automatically being made as to what defense mechanisms will occur. This effort to limit the motives and mechanisms may be responsible for much of the confusion in the author's efforts to define what is meant by each rubric. Even if there were a *perfect* correspondence between source of danger and type of defence mechanism, which the author does not claim (p. 117), the false assumption of unitary motivation would make it difficult to find case material to exemplify the mechanisms.

ROBERT R. SEARS.

Yale University.

BECK, SAMUEL, *Introduction to the Rorschach Method: A Manual of Personality Study.* Monograph No. 1 of the American Orthopsychiatric Association, 1937. Pp. xv+278.

Considering the great interest which the Rorschach method has stimulated in the last few years in this country, the publication of Dr. Beck's book will be highly gratifying: first of all, because an English translation of the original publication of Rorschach does not exist; second, because Rorschach's book is written in a language which causes difficulty even for the German reader, and more so, of course, for the English student. Furthermore, it must be borne in mind that Rorschach's book was published years ago and that in the meantime the method has been examined as to its usefulness in various directions and has been modified. Among those to whom we are indebted for the broader application and modification of the method, Dr. Beck takes first rank. The book is not only a review of the up-to-date experiences with the Rorschach test but reflects the author's own valuable results. For the author this method is not to be considered as a mere test, a technique. "It is," he writes, "an art, in which inkblots are instruments in the same sense as the sculptor's chisel, the artist's pen."

It is understandable that Beck has written a book which cannot be used in a schematic way. That does not make for easy reading, but it offers a unique value. The book is written not so much for the beginner as for the experienced student, and the latter will find in it an abundance of material valuable for application of the Rorschach method to normal and sick people. The technique, in some direction deviating from the original, is elucidated on the basis of extensive records. The expert will read with great interest the detailed interpretation of the individual responses. There is no

doubt in the reviewer's mind that this is a publication which is highly recommendable, although the pertinent literature could have been considered somewhat more carefully.

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WHITE, W. A., *William Alanson White: The Autobiography of a Purpose.* Garden City, New York: Doubleday, Doran and Company, 1938. Pp. xix+293.

Every specialist in abnormal psychology and every student of mental hygiene will want to read this book. It is a good book, although not a great one. As a biography it may never receive much attention and will doubtless be soon forgotten; but as the story of one who has figured prominently in the development of abnormal psychology and psychiatry it will live in the minds of those who know those fields. There is a curious inability to recall much from the author's experiences as a child and as a youth. This the author often comments upon but nowhere seeks to explain. The style is clear but never brilliant. A very useful bibliography of the author's published writings is included, but there is no index and perhaps there is no need for one. Nevertheless, the recollections and observations and experiences of a psychiatrist and hospital superintendent, whose life was contemporaneous with the development of modern psychiatry, make instructive and fascinating reading. The point of view presented, familiar to all who are acquainted with contemporary psychiatry, is illuminated and emphasized by the presentation of many interesting cases. For this reason alone it should go on the required lists of all courses in mental hygiene.

EDMUND S. CONKLIN.

Indiana University.

FISHER, V. E., *An Introduction to Abnormal Psychology.* (Revised Edition.) New York: The Macmillan Company, 1937. Pp. xii+533.

The purpose of this book is to meet the needs of the beginning student in abnormal psychology. The standard topics of abnormal psychology are dealt with in six chapters on the psychoneuroses and psychopathic personality, three chapters on the functional psychoses, and discussions of the organic psychoses, sleeping, dreaming, hypnotism and suggestion, the feeble-minded, and mental hygiene.

The first eight chapters comprise an orientation to the above materials by consideration of the following topics: the normal personality, personality variants and their measurement, criteria of personality adjustment, and common modes of reaction to difficulty.

The approach is essentially psychological, the author stating in his introduction that two points of view will be emphasized: abnormality is a relative matter, and mental abnormalities can be dealt with as disorders of the personality. Unfortunately, the author has so completely committed himself to a psychological approach in contrast to an organic, physiological, or pluralistic psychobiological one that a balanced appraisal of the problems of mental abnormality is rendered impossible. Consider the following, which is from the introductory paragraph of the chapter dealing with obsessions:

"An attempt has been made recently by no less an authority than Pavlov to give a physiological interpretation of the obsessions. The reader may rest assured, however, that an obsessional neurosis is a true personality disorder and that only a psychological approach to an understanding of it is possible at the present level of our knowledge of psychology and physiology" (p. 211).

The reviewer wonders why, except to bolster a previously assumed position, the reader should wish to be reassured that the obsessional neurosis is a true personality disorder. If one is seeking an understanding of the neuroses, it is not of consequence whether explanations are functional or organic, although we may arrive at an evaluation of these points of view as a result of study. A more catholic understanding and sympathetic treatment of the diverse points of view of current psychiatry, including the psychoanalytic, would enhance the value of this book as an introductory text.

Much is left to be desired in critical thought and careful writing. Paragraphs and sentences are interjected without regard for the thought structure, as, for example:

". . . It is important for the student to know that the degree or level of intelligence is not a prime factor in the development of personality disorders. *It is quite true, of course, that if intelligence were fully developed or 'unfolded' at the time of birth it would undoubtedly affect very materially the incidence of personality disorders in adult life. No one can say, however, whether the incidence would be lessened or increased.* We can only point out that some of the most brilliant minds find their way to mental hospitals whereas idiots and imbeciles are not prone to develop personality disorders: . . ." (p. 7).

The italics are the reviewer's; he questions what this material, over and apart from the possibility of determining its truth, contributes to the rather straightforward statements which precede and follow it.

Such instances are not rare and together with the straw men of the type: "many writers in psychology confuse . . . but (the author will set you right)" constitute too frequent distractions.

Careful reading of the manuscript by the author and by the publisher should have modified the following:

". . . The first fact is that any given human individual represents a limited amount of energy, that is, a limited capacity for energy expenditure. The individual is an energy transformer, it is true, but he is limited in his capacity to transform energy and therefore the amount of energy which he may expend is limited. Or, as certain writers have stated the matter, the mind is a closed energic system; the mind is capable of only a limited expenditure of energy. Translated into psychological values this means that an individual who has come to be *wholly* interested in one fact cannot possibly be interested in any other fact at the same time. A person, for instance, who is wholly interested sympathetically in one individual cannot become at all sympathetically interested in a second individual at the same time without a diminution of his interest in the first, unless, of course, he mentally combines the two individuals and reacts to them as to one" (p. 16).

". . . *Finally*, we may mention the fact that some individuals report dreaming a great deal, while others say they never dream. It hardly appears that this is literally true. Perhaps, the latter simply do not recall their dreams; we all know how readily we tend to forget our dreams" (p. 403).

Or, perhaps, *the latter* did not have any dreams to recall and *we all* readily tend to wishful thinking.

"The present writer has observed in his teaching that many students are greatly helped in the efforts to harmonize the radically different points of view and the differences in emphasis which they encounter in different books on psychology as soon as they grasp clearly the fact that any writer in psychology inevitably has a personal point of view toward human personalities and human reactions, and that what he writes is always a reflection of his own personality make-up. This is particularly true in the field of interpretive psychology."

This is an excellent point but one wonders whether the author clearly grasps it when he continues:

"Of course we psychologists would like very much to attain a truly impersonal attitude toward our subject matter, for the sake of scientific integrity; but honesty forbids us to pretend that we ever actually do it, except, perhaps, in such fields as *comparative psychology* and the simpler aspects of experimental psychology."

The italics are the reviewer's; comparative psychologists should not feel flattered.

The mechanics of scholarship could be improved. In discussing

the efforts of the introverted individual to assume the behavior of the extrovert, the author advises:

". . . He should not cast aside too readily the great introverted teachings of the past: 'Silence is golden,' 'It is a wise man indeed who says what he has to say, and when he has nothing to say, is silent,' etc.[1] "

"[1] These quotations are cited from memory and the writer cannot vouch for the correctness of their wording" (p. 41).

These quotations could have been verified with little more effort than that required to append the footnote; similarly, it is desirable to make reference to original sources of experimental studies rather than to other elementary textbooks (p. 12). It is the reviewer's opinion that teachers of psychology are entitled to textbooks that embody more careful thought, writing, and scholarship. Better texts are available.

The present book serves to emphasize a problem of increasing importance for the future relations of medicine and psychology. Study of the case histories in *An Introduction to Abnormal Psychology*, presented as arising from the author's clinical experience, suggests activities which border upon, if indeed they do not lie within, the practice of medicine. There is little in the present volume to give the student a clear understanding of the respective rôles of the psychologist and the psychiatrist and much that contributes to the psychiatrist's feeling of uneasiness concerning psychologists. It is doubtful if this book will further closer working relations between the two fields.

CARLYLE F. JACOBSEN.
Washington University School of Medicine, St. Louis.

VALENTINE, C. H., *The Treatment of Moral and Emotional Difficulties: A Practical Guide for Parsons and Others.* New York: The Macmillan Company, 1938. Pp. xi+148.

A number of books have come from England which have discussed in a most constructive way the possible relations between the pastor or religious worker and the consulting psychologist. These books have, as a rule, been written by clergymen who have studied and worked under medical psychologists and some of them bear introductions written by physicians.[1] Two things are apparent: (1) the willingness of the clergyman to give definite time and hard study to the psychology of personality—or applied psychology in the field of personality problems, and (2) the hospitality of the medical

[1] See *Psychology and Life* by Leslie Wetherhead, the present preacher at the City Temple, London.

psychologists to help from the religious field. The very term "medical psychologist," frequently used in England, well describes a branch of medicine which deals with personality and behavior disorders and seems to give more attention to the part that psychology of normal human behavior may play in the solution of personality problems than psychiatry and psychopathology are accustomed to do in this country.

The book under review is written by a lecturer on religious themes and a member of an important English diocese, and is intended as an aid to the clergy in matters concerned with pastoral work among people suffering from emotional and spiritual problems. There is an introduction by the head of the Tavistock Clinic, a mental clinic founded some years ago in London by Dr. Crichton-Miller, whose books are well known in this country. Dr. Rees acknowledges in this introduction that "the minister of religion, the teacher, the social worker, can never avoid dealing with neurotic people and their problems . . . every minister of religion should have an interest and a point of view about these conditions which will enable him to act as a first-aid station; this would be sufficient for many people, and would act as a clearing-house for those with graver conditions." There is also an introduction by a prominent clergyman and both introducers and the author agree that for the best work there must be close coöperation between physician and spiritual adviser and that the religious worker must always be sure, before beginning his own treatment, that the medical history of the patient is fully known and the medical attention complete.

The book is brief, clear, and constantly illustrated by excerpts from case histories. Recognition is always given to the fact that knowledge is needed before help can be given. A good heart and common sense are not enough. Several points stand out. Perhaps first in importance among these is the differentiation by the author between analysis and synthesis. Psychoanalysis is never decried, but the fact is pointed out that in many cases a full analysis is not needed but, rather, a synthesis of all constructive factors in the picture, both personality traits and environmental assets. This reminds the reviewer of a statement by Dr. Oskar Pfister, a clergyman, psychoanalyst of note, and close friend and student of Sigmund Freud. He describes brief and prolonged treatment and asserts that in difficulties which have arisen in the recent past a prolonged analysis is not indicated, but two or three interviews may be sufficient.

Again, in discussing "repression" Dr. Valentine states that "the essence of a repression is that it is an *unconscious* struggle. The way to end the conflict is to make it a conscious struggle" (p. 82). Here, the author's special contribution is not the bringing of the matter into consciousness—that, of course, is elementary—but the recognition of the value of struggle.

Another point, which is more controversial and which differentiates the clergyman as therapeutist more definitely from the physician or consulting psychologist, is the description of the emotional relationship. His chapter on "Personal Relationships" (Chapter VII) is one of the most valuable in the book. "The mistake that many people make," he says, "is that they try to keep emotion out of their dealings with other people." "What is needed in all our personal relationships is not less emotion, but more emotion . . . It must be emotion that is harnessed, controlled and guided; emotion that is broken in for team-work." The life of the patient "gains its significance and its fulfilment through the achievement of joyful and loving relationships with individuals." There is nothing suggestive of sentimentality here.

Finally, there is the definition of a true and mature religion, one that is a challenge to fuller living, not a retreat from struggle. It must be, the author says, a religion of love and fellowship, not one of power and domination. The value and the dangers inherent in "forgiveness," in the concept of the "Fatherhood of God," are frankly dealt with. There must be no evasion of the realities of evil. Such a religion may well be the final step in the therapy of emotionally distressed persons.

The author has benefited, apparently, by study of the books of Shand and McDougall, and his psychology has been influenced by their teachings. This might lead to differences of opinion among American psychologists. They seem to this reviewer of small importance. The author has pointed the way to a complete therapy. It is not a way of ease but of close and devoted study and definite efforts in the field of coöperation between professions—sometimes hard to bring about, but, to many sufferers, of infinite importance.

ELEANOR HOPE JOHNSON.

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NOTES AND NEWS

DR. J. B. STROUD, formerly head of the department of psychology at Kansas State Teachers College, began his new duties as associate professor of education and psychology at the State University of Iowa on September 1.

DR. F. L. WELLS, formerly in charge of the psychological laboratory at the Boston Psychopathic Hospital, has accepted the position of psychologist in the department of hygiene at Harvard University.

DR. HERBERT H. JASPER, formerly assistant professor of psychology at Brown University and director of the psychological clinic and neurophysiological laboratory, Emma Pendleton Bradley Home, has been appointed lecturer in neurophysiology at McGill Medical School and at the same time will be in charge of the new department of electrophysiology in the Montreal Neurological Institute.

DR. L. A. PENNINGTON, who has been instructor in psychology at the University of Arkansas, is now assistant professor of psychology at the University of Illinois. The instructorship at the University of Arkansas is being filled by Stanford C. Ericksen.

Nature writes: "Among the distinguished scientific men of science who have left Vienna since the *Anschluss* is Professor Sigmund Freud, who has taken refuge in London. Professor Freud celebrated his eighty-second birthday on May 6 last. His name will always be associated with the development of psychoanalysis, and the significance of his contributions to psychology was acknowledged by his election in 1936 to foreign membership of the Royal Society, but Professor Freud had not hitherto been able to sign the roll of membership. Although now resident in London, Professor Freud was prevented by infirmity from attending the society's rooms for this purpose, and although the charter book is rarely removed from Burlington House except when it is taken to Buckingham Palace for the signature of the King as patron of the Society, it was decided to extend the privilege to Professor Freud. Accordingly, on June 23,

Sir Albert Seward, foreign secretary, and Professor A. V. Hill, one of the secretaries, accompanied J. D. Griffith Davies, who as assistant secretary has custody of the charter book, to Professor Freud's residence, where the roll was duly signed by Professor Freud in the presence of his daughter, Dr. Anna Freud, and Princess Marie of Greece, who was a pupil of his and received him when he left Austria. On behalf of the Royal Society, Sir Albert Seward presented to Professor Freud an inscribed copy of a facsimile of the society's charter book."—*Science.*

THE Seventh International Management Congress, convening in the United States this year for the first time, held its five-day series of general and technical sessions in Washington, D. C., September 19 through September 23, with approximately two thousand leaders in the field of management from this country and more than twenty foreign nations attending. At this session of the Congress, the themes of "recent developments in management" and "economic and social aspects of management" were carried out through the extensive program.

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